UNIVERSITY OF LONDON
INSTITUTE OF ARCHAEOLOGY

# CONFERENCE

## ON THE

# PROBLEMS AND PROSPECTS OF EUROPEAN ARCHAEOLOGY

HELD AT THE
UNIVERSITY OF LONDON, INSTITUTE OF ARCHAEOLOGY
SEPTEMBER 16TH—17TH, 1944

PUBLISHED AS OCCASIONAL PAPER No. 6
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# FOREWORD

The Conference on the Future of Archaeology, held at the Institute of Archaeology in August, 1943, was concerned mainly with problems of organization which would affect archaeology in the post-war period. The Conference on the Problems and Prospects of European Archaeology, of which the Report is contained in the present publication, was designed to deal with more purely archaeological matters, and to discuss the problems to which the attention of archaeologists should be directed when the resumption of work became once more possible.

The purpose of the Conference was threefold, to discuss the present state of knowledge on European archaeological problems, to formulate on this basis the questions which will most urgently require attention in the post-war years, and to lay emphasis on those problems for the solution of which international co-operation is necessary. The problems of archaeology are not bounded by national frontiers, and the presence in England of a number of distinguished archaeologists of different nationalities provided an exceptional opportunity for discussion which it was felt would be useful to workers in many fields and in different countries.

Since the aim of the Conference was to view the problem as a whole, the speakers were asked to approach their subjects from the angle of questions affecting whole regions and not only particular countries. It was felt that thus could the need for international collaboration best be indicated and attention be focussed on subjects in which general progress is held up since they have not attracted attention recently in certain countries.

The Organizing Committee of the Conference was international in composition, though the members serving on it did so in their private capacities, and not as representing official bodies. One hundred and fifty-eight persons attended the Conference, including representatives of many allied countries in England at the time.

The papers read at the Conference are published in full, together with summaries of the contributions to the discussions which followed them.

# CONFERENCE

ON

# THE PROBLEMS AND PROSPECTS OF EUROPEAN ARCHAEOLOGY

SEPTEMBER 16TH-17TH, 1944

## INTRODUCTION TO THE CONFERENCE

*Chairman :* SIR CHARLES PEERS, C.B.E., F.B.A., F.S.A.,
*Chairman of the Management Committee, University of London Institute of Archaeology*

*Speaker :* PROFESSOR V. GORDON CHILDE, D.Litt., D.Sc., F.B.A., F.S.A.,
*Professor of Prehistoric Archaeology in the University of Edinburgh*

### CHAIRMAN'S REMARKS

A little more than a year ago we met in this room to inaugurate a Conference on the Future of Archaeology. At that date, after four years of anxiety and many disappointments, it seemed that the war was really turning in our favour, and we could speak of Orel and Catania as landmarks in progress. Far greater events have now put these names out of our minds, and with a sense that the night is far spent and the day at hand we meet again in confident anticipation that it is not now out of place to discuss, on a certain footing, the problems which the post-war years will bring. So the prospects for European Archaeology, a science which knows nothing of frontiers, are to be discussed to-day and to-morrow at a series of short sittings, in which the speakers are allowed a bare half-hour to say what they have to say, and a further half-hour is to be given for discussion.

So with these limits in mind the duty of a Chairman is clear, to be not merely brief but laconic. Add to this that our printed programme describes this Conference as informal and concerned with facts and theories alone. The verbal distinction you will all note and appreciate. The introduction to the Conference is in the entirely competent hands of Professor V. Gordon Childe, who now has what is left of our first half-hour to say what he pleases, so I now call upon him to do so.

PROFESSOR GORDON CHILDE

The Conference meets happily when Europe is on the point of deliverance from a long nightmare. If archaeology has been only the victim during its climax, it has not been so completely disconnected with the premonitory spasms. This is an unpleasant theme to begin upon, but I cannot pass it over in complete silence. The contributions to Nazi ideology extorted by Dr. Frick and his fellows from perversions of archaeology need now not be recalled. But I must remind you that this sort of distortion did not start with the foundation of the Third Reich. It was nearly as bad under Kaiser Wilhelm. I suspect it began well before Kossinna. Throughout the nineteenth century German historiography and prehistoriography had a definite tendency. I do not suggest that this tendenciousness was the result of any conscious bias or imposed deliberate misrepresentation of archaeologist or historian. It was the result of social selection gradually establishing a pure tradition as selective breeding may establish a pure strain of fowls. Any professional body is subject to such selection, and we all know how subtly it operates. Neither I nor anyone just now is in a position to judge how far the German nationalist tradition was responsible for selecting the facts stressed by German archaeologists or how far the facts determined the tradition. What is clear is that all accounts of European archaeology have been profoundly influenced by German theory as a consequence of the great bulk of German literature and its real technical merits. Outside Germany some schools have succumbed and accept the German standpoint. The English and Czechs have been hardly less impressed, but have reacted negatively, accepting German assumptions but trying to draw the opposite conclusions from them. Our Soviet colleagues have gone further, rejecting the assumptions too; but still as a reaction, and a reaction tends to produce distortion. I cannot altogether avoid the feeling that Soviet prehistorians have gone further than was necessary in their revulsion from German theory. Their rejection of prehistoric migration is not inspired by any text in Marx and Engels known to me, but is, I suspect, an addition to the Materialist Conception of History prompted by special conditions of international politics, a counterblast to ideological imperialism.

People said the same about German anthropology, history and prehistory during the last war, but at its end most of us became more or less pro-German and ceased to believe " propaganda ". I do not know how the younger generation feel to-day, but this time I am convinced. Therefore I want to insist that German theory and interpretations must be treated with reserve, and carefully distinguished from the facts that German science—I mean, science in Germany—has brought to light. Happily that is not too difficult. Save for Reinerth and a few more recent Nazis, German archaeologists honourably refrained from suppressing or distorting facts. They—even Kossinna—told you all they had found or observed, however incompatible with their theories.

That brings me to a second and more pleasant introductory consideration. Archaeology—*sensu stricto*—deals happily with concrete physical objects. As such

there can be no disagreement about them. There they are for the enlightened student to handle and observe. A stone axe or a water mill ; a mosque or an abbey ; an eikon or a korē *qua* material embodiments of human ingenuity and art are the same for Swede or Bulgar, Christian or Mohammedan, just as are the objects of other sciences—an eclipse, a platypus, a streptococcus, or a block of andesite. This very fact fits them to be a basis for international co-operative study in a way that the characters of political, military or ecclesiastical history can never be.

But more, most objects of archaeology are sensibly parts of the common heritage of humanity as a whole, not of any single nation or political group. I mean to say, all civilized peoples at least—all Europeans and Americans—have equally benefited by the development of mills, from the saddle quern to the steam flour mill. It is our common interest to trace the development of this part of our common heritage. The great monuments of art and architecture—the statues of Mycerinus, the Hermes of Praxiteles, the Parthenon, Hagia Sofia, Ely Cathedral, Peterhof—any educated man can appreciate and enjoy these. They are in no wise restricted to Greeks, Englishmen or Russians. On the other hand, Shakespeare, Goethe, Ibsen and Pushkin can never mean quite the same to Russians, Norwegians, Germans and Englishmen. They produced literary masterpieces, but no translation can ever catch the full perfection of the original.

Archaeology is a branch of history, one of the humanistic disciplines. But its concrete, substantial and objective character makes it a social science too, that is to say, an aspect of human activity that is the same for all humanity—a true universal, and therefore peculiarly a basis for amicable international co-operation.

The trouble of course is that such archaeological facts, in early periods at least, are so few and fragmentary that we are almost obliged to fill them up and connect them by subjective speculative interpretations. These may easily turn into metaphysics, into hypotheses, that cannot possibly be tested by experience and controlled by observation. Prehistory, and even history, easily degenerates into such.

This degeneration is enormously encouraged by the vast discrepancies in the investigation of the several natural or artificial areas of Europe. The areas of archaeological exploration in fact coincide with political divisions, for the study of antiquities is nearly everywhere more or less dependent on Government funds, and in any case is always a function of the social and economic development of a political society, i.e. of a State. Therefore the elimination of metaphysical speculations that are liable to nationalistic or other distortions and the substitution of objective archaeology and legitimate controllable deductions therefrom must depend upon international co-operative effort. It means in the long run the elevation of the standard of archaeology in every area, to that ruling in the most progressive.

Perhaps then the best introduction to this meeting will be to indicate some of the complexes of facts that at the moment provoke speculations particularly because of the politico-geographical inequalities in their distribution.

To start at the beginning, the establishment of a reliable geological basis for

the sub-division of the pleistocene is essentially an international task. We have two possible chronometers—glaciation and land movement. For the Alps, the Penck and Brückner scheme still seems acceptable, though German investigators have duplicated most of these glaciations, and triplicated Würm. But the correlation of the Alpine glaciers and the Scottish and Scandinavian ice sheets is still in a very speculative state. In the solution of this problem Switzerland, Belgium, Germany, Poland and the U.S.S.R., are immediately and jointly concerned. In the last country some novel ideas have been advanced that need critical examination by its western neighbours. But the ice movements may be capable of correlation with the changes of land-sea level familiar from Déperet. It is obvious now that river terraces and their contents cannot simply be explained in the terms either of the Penck and Brückner or the Déperet scheme alone. The correlation invites contributions from all maritime countries, especially Portugal, Spain, France and Italy.

With regard to the contents of the frame to be established, we need to know whether there is really no handaxe culture in " trans Chellasia ".[1] If not, were Hungary, Moravia and the Russian plain uninhabited? Otherwise what culture corresponds to the Lower Palaeolithic? From Saxony and Silesia come some suggestive hints, but they want further testing. What is the real truth about Menghin's Lower Palaeolithic *Knochenkulturkreis*—the real age and the affinities of the Swiss and Franconian settlements? The range of the Mediterranean Palaeolithic is better known, though it has recently been extended by Garrod's discoveries in Bulgaria and Russian work at Derkul and further north even to the Urals. The Balkans are still largely blank—notably Serbia—and the age and range of the Krapina type needs much further study.

For the Upper Palaeolithic the range, economy and origins of the Solutrean has once more become a leading issue as a result of Pericot's work at Parpalló. Twenty-five years ago Breuil's *Voyage paléolithique en Europe centrale* was responsible for the popular theory of an east European cradle. But excavation in Rumania has now produced very little Solutrean, and in the U.S.S.R. there is nothing typical at all. Plainly, no hypothesis should be formulated without more evidence from Russia, Rumania, Hungary and the Balkans on the one hand, the Peninsula on the other. And much the same applies to Garrod's Gravettian and Aurignacian. A few points on a small scale distribution map suggest bold generalizations and point where the theories may be put to the test. It is for our colleagues in Poland, Moravia, Serbia, Croatia and Slovenia particularly to conduct the crucial experiments. Incidentally, we need to know a lot more about the climate, flora and fauna both of Central Europe and Spain during the late pleistocene. I think we are too apt to apply there the Siberian picture deduced from French and Cantabrian caves. That at once raises a purely cultural question. Did Upper Palaeolithic men have no wood worker's tools, no axes or adzes? The Russians assert they did. A re-examination of collections with an open mind might repay the trouble.

[1] To use the happy term introduced by Sollas in his *Ancient Hunters* for the area where handaxes do not seem to occur.

Anyhow I can point to Lyngby axes from Moldavia, Hungary and Moravia. Are there no more ? And what were they used for ?

The word Lyngby was intended to point on to the Mesolithic problem complex in its dual aspect—the first colonization of the frozen north and the adjustment of culture to the new environment. In twenty-five years, co-operative effort has done a lot to fill in the great hiatus and also to clarify the environmental conditions that dominated it. The climatic changes and the forest sequence in N. Europe, hardly guessed in 1914, are now tolerably clear. Of the contemporary changes in Central Europe and the Mediterranean lands we are horribly ignorant. In N. Europe again several cycles of mesolithic culture have crystallized out and been submitted to a rapid process of subdivision. But in Central Europe little has been recorded, in the Balkans nothing at all, in France and the Iberian Peninsula the picture is terribly lopsided ; and very recently the seemingly reliable North has given our pretty theories two rude shocks. On the one hand the recognition of the 2nd Littorina transgression has upset the date of many sites and transferred the representative Ertebølle site of Brabrandsø from Atlantic to early Subboreal times, or in terms of local archaeological divisions from mesolithic to full neolithic ! On the other hand, Westerby claimed in 1938 to have found pottery in an early mesolithic Boreal context at Aamose on Zealand, and from what I hear, examination by the Danish National Museum under Mathiassen has confirmed his view.

English prehistorians of my generation approached the Neolithic not quite with open minds but profoundly influenced on the one hand by hang-overs from the only recently exploded hiatus dogma, on the other by the eloquence of Elliot Smith and Perry. Somehow or other, we just had to bring neolithic farmers from the East Mediterranean to Denmark, Britain and Brittany. Soviet prehistorians with other prejudices, on the contrary, have tried to show how during the mesolithic, some palaeolithic tribes gradually took to sedentary fishing and collecting, while others turned to hoe cultivation. Now our hang-overs are pretty feeble arguments —the most plausible relate to animals and plants. But really the faunistic argument is rather thin. Late pleistocene sheep are reported in S.E. Spain, the Crimea and Rumania. It would be worth looking to see if there aren't more. On no theory indeed was *Triticum dicoccum* nor *vulgare* native to Europe, but *T. monococcum* is reported to grow wild in the Balkans. Moreover, there are those inconvenient hints, that we diffusionists consistently suppress, " le tas de blé " at Mas d'Azil, the sheep from the MacArthur Cave !

If mesolithic pottery be really established in Zealand, it may turn up elsewhere too. So one element of the neolithic complex would seem to develop autonomously. That is certainly true of the former type fossil : the ground stone axe was familiar at Kunda in Boreal times. I would suggest more cordiality to mesolithic stock and grains and fresh appeals to palaeontologists and palaeobotanists.

The diffusionist is handicapped in practice by the uneven exploration of Europe. It is in Britain and Denmark, central Germany and Bohemia that years of meticulous excavations have yielded imposing sequences of cultures. Their impressive bulk

and length lends a perhaps spurious appearance of antiquity to their neolithic beginnings ; South Russia, the Middle and Lower Danube basins, the Apennines and Iberian Peninsula are far less completely explored. On this theory the neolithic should begin earlier there. In S. Russian *kurgans*, the archaic artefacts of unalloyed copper spread over the three pre-Scythian phases (*yamno*, catacomb and *grubno*) might correspond to the Central European and British Bronze Age phases but would not lead back to at all an early neolithic starting point. From Bulgaria and Serbia we have no series of bronzes to set beside the serried ranks of axes, swords and spearheads that crowd Danish and English museum cases. In the Iberian Peninsula the Middle Bronze Age and Late Bronze Age are equally defective. From Hungary, we have had lots of bronzes to fill these gaps ; only since the last war have we learnt to distinguish three or four earlier neolithic or chalcolithic stages, but the Iron Age still seems to begin with La Tène, so that the whole superimposed structure can in theory be brought down. In Italy Säflund's study of the *terremare* and Brea's re-excavations of the Ligurian cave of Arene Candide, offer hints as to how the gap might be filled.

To balance the apparent discrepancy in the south, we have been busy telescoping the northern sequences. No doubt there has been plenty of room for deflation in the schemes that Montelius and his imitators have left. I need not remind you of the prolonged overlaps between the typological periods that British, Danish, Swedish and even German prehistorians have revealed and admitted. But there are limits. Here, too, we have had surprises—once again from Denmark particularly.

When the truce began Nordmann and I thought we had effected a substantial deflation by pricking the bubble of Montelius' Neolithic I and eliminated altogether his first period of New Stone Age. Now our success is not so certain. Brøndsted has come along with the Virrings culture—contracted burials accompanied by cord-ornamented pots, some of which bear grain impressions, that may be pre-Dolmen. In Swedish dwelling-places Bagge has found a culture similarly defined by cord-ornamented pottery, sometimes stratified below Ertebølle occupation and everywhere older than the Swedish counterparts of the classical Eurasian pit-comb ware with its Peterborough affinities.

Megalithic tombs formed the springboard of the English revival of diffusionism and certainly constitute a thoroughly international subject. I have only one remark to make : Daniel has, since the war, reduced the diffusionist position to a rigidly logical typology with the *tholos* as the basal form. Admittedly there isn't much furniture available to control typology by. *All* that there is, is dead against the thesis. The segmented bead from Parc Guérin points to the same late date as the Wessex types of pendant from Carrowkeel, L. Crew and Alcalá. The unifacially ribbed blades from Alcalá and Los Millares can just as well be compared to those found in Angus and Wexford, with late Cinerary Urns, as to that from Bygholm—anyway not so well dated as we used to think.

The diffusion or autochthony of neolithic economics in N.W. Europe illustrates

a class of problems the solution of which depends upon a reliable chronology in *all* the varied provinces of our Continent. Precisely similar problems are raised by the origin and diffusion of metallurgy, the urnfield invasions of which we hear so much, the origin of the La Tène culture—or cultures—and so on. I might refer to Witter's apparently well-documented thesis on the development, autochthonous in Central Germany, of a school of metallurgy using standard bronze made from local ores out of an equally autochthonous copper age. I am frankly so suspicious of a Nazi's honesty that I have never really faced up to his arguments. The real and definite verdict of science on this and the other issues must again await the establishment of a reliable frame applicable to the Continent as a whole. The first condition for that is the satisfactory definition of the culture sequences in the defective provinces I have already named. Till such are available, speculations on these questions of origin and so on are just metaphysical. The answers, I mean, cannot rest on observed facts but are determined by *a priori* assumptions. A continuance and elaboration of this discussion would therefore only serve to point out the need for more and better *excavations*. I think it better therefore to turn to some problems that can partly be solved by a more critical study of existing data in the light of new ideas that had developed in the truce and even during the subsequent war.

Only ten years ago Curwen revealed new possibilities in corn-grinding implements—a class of relic common enough and humanly most important, but normally dismissed with vague phrases in excavators' reports. Firstly he distinguished grain-rubbers in which a bun-shaped stone was moved round and round with one hand from saddle querns where the sausage-shaped upper stone was pushed to and fro with both hands. He established the priority of the former type in Britain. I amused myself during the black-out with trying to discover whether his conclusions applied to other provinces; I got a certain way with the published finds, but the evidence was too sparse and vague for any conclusion. Continental museums are full of specimens, many fairly well dated, that are just waiting to be re-examined from Curwen's standpoint.

In the same paper Curwen worked out the typology of rotary querns for Britain and offered some suggestions as to the origin of the device. Two years ago I searched the literature for clues to fix the origin more precisely and show the diffusion of the invention. I found enough evidence to publish a short paper, but I was horrified at the inadequacy of the publications and the total gaps in the record. In many regions—notably the Balkans, Hungary, Upper Italy and S. France—the first appearance of such implements could not be dated at all. In still more, the precise form of the mill was indeterminable. Curwen's typology demands cross-sections of the upper and nether stones, but it is rare to find these sections reproduced. Now the invention of the rotary quern is one of the major events in the history of technology ; it must have taken place in full historical times, though it reached barbarian Europe before literacy; and the chronology of the relevant period—the La Tène—is fairly precise. The quern then would

form a test case for diffusion. We ought to be able to plot datable examples in zones round some centre or centres. We ought to be able to define routes. I still think the rotary quern may be able to rehabilitate for its age the somewhat discredited Atlantic route. I expect material for all this is lying about in dusty cellars in every European country, and only needs re-excavation from this thin modern dust.

To the same context belongs another still more epoch-making invention, the water wheel, or rather water wheels. The origin and diffusion, too, of this invention falls in full historical times, but the literary evidence is inadequate. Archaeological evidence is hard to come by, I confess, but not unobtainable. In the last decade two actual water wheels and a good picture of a third have been recovered by scientific excavation. These are all horizontal. There is no reason why others should not come to light in Late Roman and Dark Age sites.

## Discussion

*Mr. Miles Burkitt.* A most interesting and suggestive talk has been given us. Not only has Professor Gordon Childe indicated the way we should go, but he has also suggested a number of questions which need solution through international effort. We must remember, however, that the standard of observation and investigation varies tremendously from excavator to excavator, and from country to country. There have been instances where statements have been published which on investigation have been shown to be simply not the facts. An investigator must exercise some imagination when interpreting his finds. But it sometimes happens that the investigator throws out a suggestion which other folk take up and pass on until finally it is stated as proved fact.

*Mr. C. F. C. Hawkes.* What is an archaeological fact? A stone axe, as we see it in a museum, is only a fact of a rather simple sort. It will not advance our knowledge very far unless supported by an archaeologist's documentation of its discovery. Then indeed it can take us much farther, provided always that we have some guarantee of the archaeologist's reliability as a discoverer. That means, above all, a guarantee of the competence of excavators. For archaeological facts in this extended sense of the term are often not so simple. They may be very complicated, and have to be fingered only with great care. The remedy, then, for the difficulties of which Mr. Burkitt has warned us, is one that lies still in the distance ahead : a universally reliable standard, among all our fact-finders and particularly among excavators, of archaeological technique.

Surely, therefore, when we speak of the prospects of international co-operation in European archaeology, we should be thinking not solely of co-operation for solving problems, but at the same time of co-operation in the field of technical method. It will mean interchange of information, of instruction, of training facilities for students, and of museum type-collections. We must all be constantly on the watch to improve our own technique of every kind of archaeological activity and interpretation. And if we in this country find that our own problems require us to make fact-finding expeditions to other countries, we must be equally ready to welcome similar expeditions from other countries here. Unless we do both these things, we shall not play our full part in the international life of European archaeology, which should be, above all, one of freely-shared endeavour to do better work. The more we all succeed in that, the nearer we shall get to a sound doctrine of archaeological fact.

*Professor Sulimirski.* I was working for several years in the south-eastern part of Poland and I had to deal with problems concerning, besides this part of my country,

the adjacent parts of Rumania and Russia. I had the opportunity of making some trips to both countries and found colleagues who wanted to co-operate. But the real difficulty was political conditions : our Soviet colleagues were not allowed to communicate with me directly. It seems to me that it is not enough to have an understanding and will for collaboration but we have also to try to find a way how to overcome such things as political difficulties.

*Dr. Huzayyin.* One thing from which students suffer is how to grasp the different techniques of study. There are different techniques in each country. In my own, Egypt, we have archaeological expeditions from various countries, and each has its own technique with very few exceptions.

One point on which we can start is that in future publications we should always publish how the site was excavated, and this of course makes it much easier to get a true picture and make a better judgment of the picture.

Another thing is that we should establish some kind of international co-operation, not for standardization but at least to bring that near ; standardization is not always a good thing. It may not be always happy to have the same technique in all countries, but if we bring techniques nearer, and if we then have one established fact, there is no harm in that being tested by different techniques.

I think that if we could give more attention to co-operation in the understanding of different techniques in different countries it would help students.

*Dr. Margaret Murray.* There should be international co-operation on the part of museums, and objects from one country should be shown in others. Many countries shut their own archaeological specimens in their own museums, and unfortunately archaeologists are not blessed with very many of this world's goods and travelling is expensive. If objects could be sent to other museums I think it would greatly simplify the study of archaeology.

*Dr. S. Weinberg.* One of the important problems which must be dealt with is that presented by archaeological excavations which have never been properly published. Many of the sites which fall into this category are very important and yielded valuable remains. Although in many cases much of the documentation will have been lost, the material still warrants careful study and can contribute much to our knowledge. Since such material remains thus far under the control of most, if not all, of the foreign schools working in any particular region, the problem would perhaps best be worked out on an international basis for each region. The important thing is to get the finds published together with whatever information, if any, remains on the conditions of finding.

# REVIEW OF THE CHRONOLOGY OF THE PALAEOLITHIC PERIOD

*Chairman :* Miles Burkitt, Esq., M.A., F.S.A.,
*Lecturer in the Faculty of Archaeology and Anthropology, University of Cambridge*

*Speaker :* F. E. Zeuner, Esq., D.Sc., F.G.S., F.Z.S.,
*Lecturer in Geochronology at the University of London Institute of Archaeology*

## A. Methods

The chronology of the Palaeolithic is exclusively dependent on the relative and absolute chronologies devised for the climatic phases of the Pleistocene. In Europe, climatic phases are evidenced by glaciations in the temperate parts, pluvials on the Mediterranean shores, and oscillations of the sea-levels around the coasts generally. The fluctuations indicated by these three groups of phenomena are linked, since the pluvial phases were roughly contemporary with the glacial phases, while the periods of high sea-level occurred during the interglacials. The sequence of glacial phases, therefore, may be used as the standard for a relative chronology. Continued research has made it necessary to subdivide the major phases, as shown in Table 1. The correlation of these climatic phases with the periodical changes in the distribution of solar radiation over the seasons has added to this relative chronology an absolute time-scale of considerable interest.[1]

The application of this chronology (which as such is entirely independent of archaeological concepts) to Palaeolithic industries is largely a matter of patient search for, and study of, those rare sites which afford a means of fitting industries into the climatic sequence. Their number is decidedly small ; outstanding ones are Swanscombe in England, Grotta Romanelli in Italy, Grotte de l'Observatoire and the Grimaldi Caves on the Italo-French Riviera, Ehringsdorf in Thuringia, Saint-Acheul in north France. To these should be added the Mount Carmel caves which are of importance in the interpretation of the European sequence. But many of the familiar Palaeolithic sites are, in spite of their typological significance, not dependable from the chronological point of view.

## B. Some Results

The more important results of chronological research are summarized in Table 2. With regard to absolute chronology, the first and last occurrences and the varying duration of cultures and industries are worth noting ; they are summarized in Table 3.

[1] For fuller information on the geological aspects of Palaeolithic chronology, see Zeuner, F. E., *The Pleistocene Period*, 322 pp., London (Quaritch), 1945 ; for archaeological evidence and succession of industries and races, see *Dating the Past*, London (Methuen) (to appear shortly).

With regard to the replacement or displacement of industries, and local survival, attention may be drawn to the following points :—

(1) In some parts of Europe, the so-called Middle Palaeolithic appears to survive for some considerable time while Upper Palaeolithic is present in others.

TABLE 1

| MONOGLACIALISM | BIGLACIALISM | PENCK & BRÜCKNER | DETAILED RELATIVE CHRONOLOGY | | ABBREVIATIONS |
|---|---|---|---|---|---|
| Ice-Age | second glaciation | Fourth Alpine glaciation "Würm" | Last Glaciation | 3rd glacial phase | $LGl_3$ |
| | | | | interstadial oscillation | $LGl_{2/3}$ |
| | | | | 2nd glacial phase | $LGl_2$ |
| | | | | interstadial oscillation | $LGl_{1/2}$ |
| | | | | 1st glacial phase | $LGl_1$ |
| | | Third Interglacial | Last Interglacial | second part of interglacial | LIgl |
| | | | | minor cool phase | |
| | | | | first part of interglacial | |
| | | Third Alpine Glaciation "Riss" | Penultimate Glaciation | 2nd glacial phase | $PGl_2$ |
| | | | | interstadial oscillation | $PGl_{1/2}$ |
| | | | | 1st glacial phase | $PGl_1$ |
| | only interglacial | Great Interglacial | Great or Penultimate Interglacial with one or several cooler phases | | PIgl |
| | first glaciation | Second Alpine glaciation "Mindel" | Antepenultimate Glaciation | 2nd glacial phase | $ApGl_2$ |
| | | | | interstadial oscillation | $ApGl_{1/2}$ |
| | | | | 1st glacial phase | $ApGl_1$ |
| | | First Interglacial | Antepenultimate Interglacial | | ApIgl |
| | | First Alpine glaciation "Günz" | Early Glaciation | 2nd glacial phase | $EGl_2$ |
| | | | | interstadial oscillation | $EGl_{1/2}$ |
| | | | | 1st glacial phase | $EGl_1$ |
| Pliocene | Pliocene | Pliocene | ? period of transition Villafranchian (Pliocene) | | |

In north France, Levalloisian has been found in deposits referable to the second phase of the Last Glaciation. Similarly, the Mousterian of the Creswell Crags would have lasted into the cold phase just mentioned. In Italy, A. C. Blanc has

TABLE II

| Time Scale | Relative Chronology | West and Central Europe | | | | Northern Mediterranean | | | Palestine | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Core | Flake | | Blade | Core | Flake | Blade | Core | Flake | Blade |
| | | | | | Mesolithic | | | Mesolithic | | | *Mesolithic* |
| 25,000 | $LGl_3$ | | | | pre-Tardenois final Magdalenian (Creswellian) | | | ? *cf. Magdal.* (Capsian influence) | | | |
| | | | | | Magdalenian (Creswellian) | | ? *Mousterian* | Magd. / Grimaldian / Micro-lithic (Capsian influence) | | | |
| 72,000 | $LGl_2$ | | Pin Hole Moust. / final Levallois | | Magdalenian (Creswellian) / Solutrean / Upper Aurign. / Middle Aurign. / Aurignacian | | Mousterian | Solutrean / Gravettian | | | Atlitian / Mid. Aurignacian |
| | | | | | | | Mousterian | Middle Aurignacian | | | Mid. Aurign. / Chatelperronian |
| 115,000 | $LGl_1$ | | Mousterian | Levall. V | | | Mousterian | | | Upper Levalloiso-Mousterian | |
| | LIgl | Micoquian / *Upper Acheulian* | Mousterian / *Tayacian* | *Levall. V* / Middle Levall. | | Acheulian | Mousterian / "Mousterioid" | | *cf. Micoquian* | Lower Levalloiso-Mousterian | level with Upp. Palaeol. facies |
| 187,000 | $PGl_2$ | Middle Acheulian | Middle Levallois | | | | | | cf. Micoquian Upp. Acheul. | | |
| | | Middle Acheulian | | | | | "Mousterioid" | | | | |
| 230,000 | $PGl_1$ | | Early Levallois | | | | | | | | |
| | PIgl | Middle Acheulian / Lower Acheulian | ? *first appearance of Levallois technique* / Clactonian II | | | (Abbevillian present but not dated) | Clactonian | | | ? *Tayacian* | |
| 435,000 | $ApGl_2$ | | | | | | | | | | |
| | | | | | | | | | | | |
| 476,000 | $ApGl_1$ | | | | | | | | | | |
| | ApIgl | Abbevillian | *Clactonian I* | | | | | | | | |
| 550,000 | $EGl_2$ | Cromerian | | | | | | | | | |
| | | Norvician | | | | | | | | | |
| 590,000 | $EGl_1$ | Ipsvician | | | | | | | | | |
| Villafranchian | | ?"*Abbevillian*" "*Sicilian*" of Morocco | ? *pre-Red Crag flakes* | | | | | | | | |

claimed that Mousterian survived even until after the second phase of the Last Glaciation but here further and more conclusive evidence, both chronological and typological, would be desirable.

(2) Upper Palaeolithic appears first in Palestine (Mount Carmel, D. A. E. Garrod) as an admixture in Acheulian and Levalloiso-Mousterian levels of the Last Interglacial. During the first interstadial of the Last Glaciation, Chatelperronian is found in Palestine, and an apparently Grimaldian industry in south Italy (Grotta Romanelli, G. A. Blanc). By the end of this interstadial, Aurignacian has spread to most parts of Europe (except where Middle Palaeolithic survives), and the Middle Aurignacian is replaced by Gravettian and related industries as the climate deteriorates.

TABLE 3

| Industry | Approximate earliest and latest dates | Duration in years |
|---|---|---|
| Crag industries .. .. .. .. | ?-540,000 B.P. | 60,000 years or more |
| Abbevillian .. .. .. .. .. | 540-480,000 B.P. | 60,000 years |
| Acheulian .. .. .. .. .. | 430-130,000 B.P. | 300,000 years |
| Middle-Upper Acheulian (excl. Micoquian) | mid PIgl-mid LIgl | 200,000 years or less |
| Clactonian (excl. Tayacian) .. .. | 540-240,000 B.P. | 300,000 years |
| Levalloisian .. .. .. .. .. | 250-70,000 B.P. | 180,000 years |
| Mousterian .. .. .. .. .. | 140-70,000 B.P. | 70,000 years |
| Aurignacian .. .. .. .. .. | *c.* 100-70,000 B.P. (longer in south) | 30,000 years |
| Solutrean .. .. .. .. .. | *c.* 72,000 B.P. | very short |
| Magdalenian .. .. .. .. .. | 70-20,000 B.P. | 50,000 years |
| Mesolithic .. .. .. .. .. | 20-*c.* 7,000 B.P. | 13,000 years |

(3) In the interstadial $LGl_{1/2}$ and especially in the following, second, phase of the Last Glaciation, industries succeed each other rapidly, i.e. every few thousand years, whilst after the climax of this cold phase, a more settled period begins, during which the Magdalenian and Magdalenian-like derivatives of the Gravettian develop quietly for several tens of thousands of years. This comparative stability comes to an end at the climax of the third phase of the Last Glaciation, when Mesolithic replaces the Palaeolithic, though microlithic tendencies have been in evidence, and spreading, for some time previously.

(4) The first appearance of the Levalloisian technique—an event of importance since it involves a greater amount of foresight in the making of tools—can be traced to the Penultimate Interglacial, both in England (Swanscombe) and in central Germany (Oberwerschen), but as an industry Levalloisian is so far unknown from any deposit ante-dating the first phase of the Penultimate Glaciation.

(5) In west and central Europe, the Antepenultimate Glaciation is represented by a gap in the archaeological record. It is conceivable that during this very first intense glaciation man was still ill-adapted to the climatic conditions of periglacial Europe and therefore on the whole avoided this zone.

(6) Prior to the Antepenultimate Interglacial with its Abbevillian and Clactonian I, only the East Anglian Crag industries discovered mainly by Reid Moir are found in datable deposits. They indicate an evolution from the Ipsvician to the Cromerian stage in the course of the Early Glaciation, and thereafter into Abbevillian (and Clactonian ?). This, together with the discovery of Abbevillian in (or on ?) supposedly Sicilian beach deposits in Portugal and Morocco, by Breuil, Zbyszewski, Neuville and Rühlmann, suggests that man the tool-maker was in existence at the beginning of the Early Glaciation and very probably prior to it. Pliocene man, therefore, appears to be emerging from the stage of pure hypothesis.

## C. Outstanding Problems

Of the numerous problems of Palaeolithic chronology (and, closely linked with it, typology) of Europe, only a few can be pointed out.

(1) Special attention should be paid in the whole of central, east and south-east Europe to early flake industries, of Clactonian or Levalloisian type. In connection with this quest, the problem of what constitutes a " prepared striking platform " of the Levallois type will have to be reconsidered, and the typological nomenclature of the numerous central European localities extricated from the pool of racial and national isolation and revised in the light of the western European evidence.

(2) An attempt should be made to fit the Chatelperronian sites of France into the detailed chronology.

(3) More attention should be paid to the *chronological* correlation of the north African Palaeolithic with Europe, on a palaeoclimatological basis.

(4) In Italy, the suggested survival of the Mousterian should be studied more fully, both from the chronological point of view (exact horizon of implements) and from the typological one (" Mousterian " as a facies of Aurignacian imposed by pebbles as raw material ?).

(5) The number of Palaeolithic sites which can be dated closely is comparatively small. It is highly desirable, therefore, that in the future the chronological evidence afforded by stratification, weathering, fauna, etc., be studied more closely and with the use of modern methods. We might then have a chance not only to improve our chronological system, but ultimately to reconstruct the movements of Palaeolithic tribes in much the same way as is now being done in the Neolithic and later periods.

### Discussion

*Dr. K. P. Oakley.* The problems of Pleistocene correlation in this country differ from those on the continent. It is much more difficult to apply absolute chronology here. For one thing we have not got loess sheets to guide us. Though we may know the glacial successions in great detail in several regions in Britain we are still not sure how they correlate with one another ; still less are we sure with what glaciations we are dealing in terms of the alpine sequence. Geologists had great hopes that archaeology would

provide them with the evidence for establishing correlations. Recently, however, it has been shown that the complexity of the palaeolithic cultural succession, with its local variations, must disappoint such hopes, and it is probable that geology will have to date the industries, rather than vice versa. One might cite the case of Bakers Hole in the Thames Valley. It was thought that a sound link with the continental sequence had been established when the Abbé Breuil identified implements from under the Coombe Rock at that locality with his Levallois I-II. Now it has been found that the industry shows Acheulian influence, which on continental analogy would make it equivalent to Levallois V. If it is really contemporary with Levallois V the associated deposit must be transferred from the penultimate to the last glaciation. Moreover the local correlation will have to be reconsidered, for the position of the deposit in the geological sequence had been largely inferred from an archaeological " fact " !

A great deal will hinge on the future study of the Levalloisian industries. Recent work in Portugal by Zbyszewski has shown that they persisted through all stages of the last glaciation. Certainly in Britain Levalloisian technique persisted into the Mesolithic. What all this means in ethnological terms we do not know. In origin the Levalloisian culture seems to have had more in common with the Acheulian than with the Clactonian.

*Mr. A. D. Lacaille.* It is not so long since Palaeolithic flakes, other than obvious waste, from Pleistocene deposits in England, and indeed on the Continent as well, were invariably called Mousterian, early or late according to their associations. Thanks to the work of British inquirers, and Breuil and other Continental workers, however, it is now recognized that such objects may not be so easily dismissed. They can be assigned to distinct cultures including the Clactonian and Levalloisian of the periglacial regions. Thus, some order has been arrived at. Nevertheless, we have a long way to go before our sequence can be correlated satisfactorily with that established in, say, the Somme valley and south-western France. We do know now that in this latter district the true Mousterian was essentially a cave-culture which developed on various stocks, Clactonian, Acheulian, Levalloisian and regional elements. It had a long chronological range and was replaced mainly by Levalloisian in the periglacial regions of northern France and England. The Levalloisian, gradually becoming better known in England where it is widespread, covers an even greater span involving three genial and at least two cold periods. It blends with Middle Acheulian at the lower end of the scale, with advanced Acheulian intermediately and at the upper end it has links with the Aurignacian. Since the Levalloisian may be said to belong to the great Acheulio-Mousterian complex, it presents a wide scope for inquiry as one of the major problems of the Pleistocene in England. The assigning of the various phases of the Levalloisian to their true geological horizons, and their correlation with the phases of the equivalent Middle Palaeolithic of the Continent can only be accomplished by the collaboration of British and Continental colleagues in the archaeological and allied fields of research. Among the scientists upon whom we count is to-day's lecturer. His contributions have been outstanding and he has given us an inspiring lead.

# THE MESOLITHIC AND NEOLITHIC PERIODS

*Chairman :* Professor Gordon Childe

## I. MAN AND NATURE IN PREHISTORY, WITH SPECIAL REFERENCE TO NEOLITHIC SETTLEMENT IN NORTHERN EUROPE.

*Speaker :* J. G. D. Clark, Esq., M.A., Ph.D., F.S.A.,
*Department of Archaeology and Anthropology, University of Cambridge*

The soil is to the prehistoric archaeologist what the manuscript room is to the historian, the ultimate source of his most precious discoveries. In reflecting on the rudimentary state of our knowledge of prehistory, even in the best explored parts of Europe, our thoughts turn, inevitably, to the soil, from which the prehistoric peoples sprang, on which they lived, and to which they have returned. Our task must be to dig, certainly, but it must also be to interpret. We have to dig, not mechanically, but with insight. We must disinter the dead, uncover their dwellings and gather the often pitiable relics of their handicrafts ; the material embodiment of their cultures must be excavated and classified, their sequence be established and their distribution plotted. All this is important, but only as a frame of reference. Within such and such a material framework men once lived in society.[1] It is the kind of life they led and the manner of their association that constitute the real essence of our study, the drama of life itself rather than details of stage properties, on the more trivial of which so many scholars have lavished their erudition. The hunters and ploughmen of antiquity were alive in a sense which the vicarious nature of modern urban life makes it increasingly difficult for most of us fully to apprehend. If we would portray their lives, we cannot remain content with a flat picture, a composition in time and space alone ; somehow we must convey the depth of reality as we know it, albeit dimly, in our own lives.

Far be it from me to deny that in seeking to reconstruct the lives of the preliterate communities of prehistory we must rely predominantly on our interpretation of the durable fossils of antiquity, the material objects and structures unveiled by archaeological excavation : regarded as social documents, indeed, these achieve an altogether new and enhanced value. What I do suggest is that we can hardly hope to learn much of the essential life processes of prehistoric communities unless we draw freely upon the whole range of knowledge, scientific

[1] See the author's *Archaeology and Society* (Methuen, 1939), chapter VI ; the same point of view determined the method of presentation adopted in *Prehistoric England* (Batsford, 3rd edition, 1945). Archaeology is regarded as only one of several methods of study capable of throwing light upon the social life of prehistoric times.

as well as humane. I think we should co-operate far more closely with the natural scientists than we have done, not merely for purposes of establishing chronology, important though this may be, but even more so for the help they can give us in giving depth to our picture. Nor would geologists and palaeobiologists fail to gain from closer association ; not a few of the phenomena, for which they seek explanations in terms of external nature, and which we are invited to accept as a basis for an objective geochronological system, may prove on closer scrutiny to be due to human activity.

The convention of co-operation with natural scientists may already be said to have been long established, particularly for the earlier stages of prehistory, which in fact could hardly otherwise exist. I sometimes wonder whether the collaboration has been quite as vital as it might have been, whether we derive half as much from it as we ought. Perhaps we have been too conscious, because of differences of training, of living in distinct worlds. We have tended to think of " man " and " nature " as though they were somehow separate. We have ignored too much the implications of the fact that man is himself very much a part of nature. The natural environment, which we sometimes tend to regard as external to man is, in fact, to a greater or less extent itself the product of human activity.

As a factor in animal or plant ecology and even as an agent in physiographic change, man has become immensely more important since he learnt the arts of domestication, and progressively so as he extended his control of energy. But it is well to remember that from the beginning he has been a more potent agent in environmental change than any other species. As self-styled Lord of Creation, *Homo sapiens* has not always cut a very splendid figure, but his dominance as a species remains, up to the present, a fact. As students of man we have been a little too modest : we have been too ready to interpret prehistory in terms of natural environment, as though this did not include the subject of our studies, and as though it was not itself moulded appreciably by human activities. In reality the natural scientist and the prehistorian study the same phenomena from the point of view of different specialisms. If we cannot comprehend man apart from nature, neither can we understand the natural history of the last few hundred thousand years without taking full account of man and his precursors. Man is part of nature and nature comprehends man. We have become accustomed, perhaps rather too accustomed, to take account of the influence of natural environment on the affairs of man. We have not paid sufficient attention to the effect of human activities in environing nature.

Yet this aspect of these interrelations between man and his environment holds great promise for prehistory. In so far as early man's activities have modified his environment, it must be obvious that we should be able to infer these activities by scientific study of the relevant scientific sequences—and in establishing temporal relevance we have geochronological methods to guide us. It should frequently happen that natural scientists working on purely natural deposits will be able to increase our knowledge of the *activities* of early man more notably than archaeologists

conducting expensive excavations on sites famous as monuments of antiquity. Now, when all is said and done, it is activities rather than things, which constitute life—and the most direct method of studying the preliterate communities of antiquity is surely to investigate their manner of living. Our approach has been too formal —insufficiently dynamic. We may be reduced to having to infer activities from things, but this is at best a roundabout method. When human life has left its record in the book of nature, let us not be shy of reading that book or at least encouraging the scientific specialists to read it for us.

To illustrate what I have been saying, let me remind you of the close relations which have always subsisted between men and the various associations of plants. I need hardly emphasize that these relations have always been reciprocal, although the importance of the human factor in plant ecology has naturally grown as the intensity of his settlement and his capacity for controlling his environment have increased. It would be tedious to insist on the role of vegetation as a factor in human settlement ; the struggle against forests, to take but one example, has been a veritable *leitmotif* in the expansion of European settlement from Neolithic until modern times.

In studying groups in a food-gathering stage of development, such as the mesolithic peoples of northern Europe, it can be assumed that, discounting the possibility of occasional restricted clearance for the purpose of temporary encampments, the density and composition of forests was regulated by factors quite outside human control. But if mesolithic man can be dismissed as a significant factor in the history of forest trees, this is not to say that he left no mark in the history of plant ecology. Everyone is familiar with the fact that organic refuse, by stimulating the growth of certain species, may for a time disturb the balance of plant life. Now mesolithic man, like the Eskimo, to the ruins of whose huts in western Greenland Mathiassen was led by luxuriant growths of weeds, was a great producer of organic refuse. The stench of unwanted fish bones and trimmings, discarded meat bones, excrement and offal must have marked his camping places long after he had moved on to fresh ones in obedience to the seasonal exigencies of his food-supply. We cannot recapture those smells, if we would, nor can we, like Mathiassen,[1] appreciate at first hand the botanical effects of the decay of organic refuse. What we can do is seek for indications of such botanical disturbances by means of pollen-analysis, since the weeds nourished by the refuse must have scattered their pollen in the neighbouring bogs. Danish palaeobotanists have, in fact, discovered that mesolithic settlements are registered in neighbouring peat-bogs by exceptionally high representation of the pollen of the *Chenopodiaceae*. During Boreal times in Denmark, these hardy annuals which require nitrogenous soil and are intolerant of shade, flourished naturally only by the seashore. On the other hand, the decay of organic material on an open camping-place in the interior would temporarily reproduce exactly the conditions needed. Thus, when the palaeobotanist finds evidence for the former abundance of the

[1] *Archaeology and Society*, p. 39.

*Chenopodiaceae* on an inland site in Denmark, he in effect guides his archaeological colleague to the neighbourhood of an ancient site. The value of this registration of mesolithic settlement in the peat-bogs of northern Europe to the student of early man need hardly be emphasized.[1]

It is sometimes assumed that the role of the scientist diminishes the later we travel in prehistory. In fact the reverse is true. From the point of view of plant ecology even a poor extensive farming culture had much profounder effects than food-gathering culture, and the more intensive farming grew, the more powerfully its influence was felt. Since north-western Europe as a whole was densely forested in post-glacial times, the expansion of farming involved first and foremost forest clearance, which under an extensive system was of a temporary, but under intensive cultivation of a permanent character. Next, cultivation of the soil and the creation of meadows for grazing livestock favoured the growth of characteristic weeds, which became, so to speak, sign-posts of farming. Again, the cultivated crops were themselves a new element in the plant ecology of any area into which they were introduced. Thus the spread of a mixed farming economy introduced changes in the composition of vegetation, which in the aggregate were of a sweeping character. Conversely, it will be evident that, in so far as we can detect and interpret these changes in the unfolding record of the vegetation of any region, we should be able to gain invaluable insight into the spread of farming, the progress of forest clearance, the standards of tillage and the varieties of crops grown during remote antiquity.

Pollen-analysis is one of the readiest means available of arriving at such results, opening up, as it does, the study of fluctuations in the proportions of the various herbs and grasses, as well as forest-trees, growing at different times. Thus, forest-clearance will obviously be reflected by a diminution in the total forest tree content per cubic unit, and a corresponding rise in that of herbs and grasses. In the case of temporary clearance, such as was practised in northern Europe during Neolithic times and most of the Bronze Age, natural regeneration of the forest will be marked by a characteristic sequence in the proportions of the various trees. Certain trees, like birch and alder, owing to their earlier maturity and power of more rapid dispersal, enjoyed an initial advantage over a slower mover, such as the oak, in recolonizing the abandoned clearings. Here, then, we have the true explanation for fluctuations in pollen diagrams for forest trees which might wrongly be interpreted in terms of climatic change or even used as the basis for geochronological dating. In actuality the brief birch maximum, so commonly met with in Danish peat-bogs at a level contemporary with neolithic settlement, often marks the passage of a community, whose economy was based on extensive, shifting agriculture.

Modern developments have shown that the method of pollen-analysis is likely to be fruitful in proportion as it is applied to a wide range of vegetation. Obviously,

[1] J. Iversen, " Land Occupation in Denmark's Stone Age," *Danmarks Geologiske Undersøgelse*, II Raekke, nr. 65, Copenhagen, 1941.

if analysis is extended to the various weeds favoured by farming economy, this should help us to estimate the role played by farming in the area in question, as well as throw light on the general standard of tillage maintained. J. Iversen, in a recent study on certain Danish bogs, has illustrated this very clearly.[1] Three weeds, in particular, he has stressed—*Rumex*, *Artemesia*, and *Plantago*. *Rumex*, the pollen of which is delicate, but identifiable under favourable conditions, is known to have been established in Denmark since late glacial times, when it enjoyed a

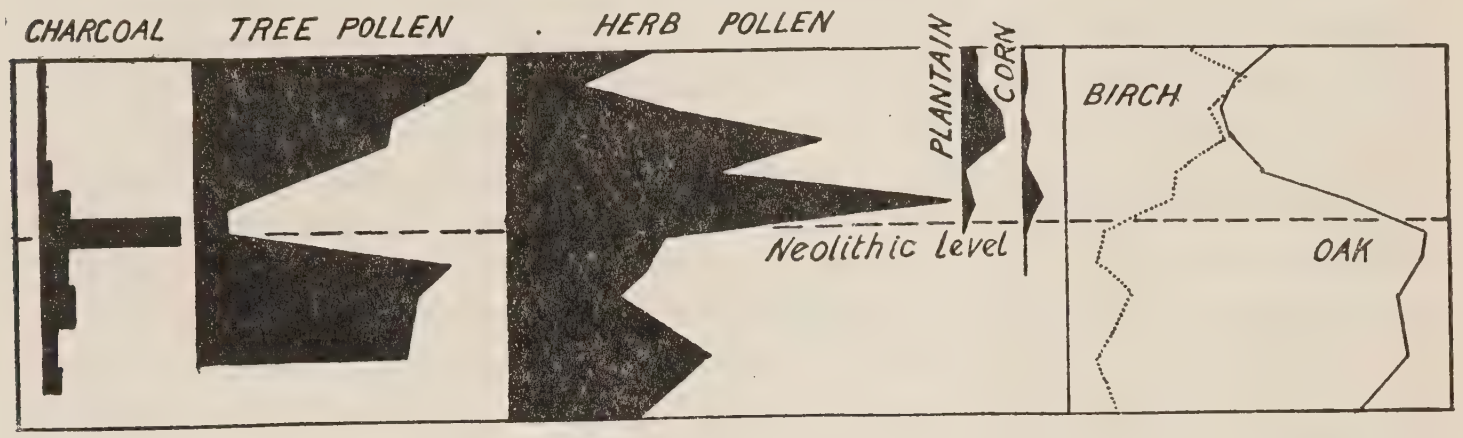


FIG. 1.
Diagram illustrating fluctuations in the frequencies of charcoal and of fossil pollen at a neolithic level in Ordrup Mose, Klampenborg Fjord, Denmark. (*After J. Iversen.*)

brief period of abundance. Since then it appears to have occurred freely only in association with agriculture on meagre soil. To-day, *Rumex acetosella* is immensely widespread as a weed on the sandy, washed-out, acid soil of mid-Jutland. The plentiful occurrence of *Rumex* pollen in the post-glacial levels of Danish bogs can be taken as a fair index that fields were cultivated in the area. It is interesting to note in passing that *Rumex* pollen has been identified in soils from Bronze Age barrows, as well as from Early Iron Age sites in Denmark. The pollen of *Artemesia*—the species is hardly ever identifiable from pollen—is another index of the existence of cultivated fields. *Artemesia* only ceased to be a troublesome weed in Denmark with the introduction of deep-ploughing. There is no doubt that the conditions which obtained under primitive farming must have been highly favourable to the growth of *Artemesia*. It also seems to be true that conditions were not otherwise particularly conducive to its flourishing in Denmark and that, in consequence, the occurrence of its fossil pollen in abnormal quantities can be taken as an indication of farming economy. Most striking of all is the plantain (*Plantago*),[2] which followed the European settlers to North America and in the parlance of the American Indian marked " the white man's trail ". Neither *Plantago major* nor *Plantago lanceolata*, each large producers of pollen, appeared in Denmark before the

[1] op cit. I have to thank Dr. H. Godwin of the Botany School, Cambridge University, for introducing me to this stimulating work and kindly lending me his copy. For Godwin's illuminating comments on Iversen's work reference should be made to *Nature*, vol. 153, 511.

[2] The spread of the plantain (*Plantago lanceolata* and *major*) into Finland was considered by K. Linkola, author of the classic paper " Studien über den Einfluss der Kulture auf die flora in den gegenden nördlich vom Ladogasee " in *Acta Soc. Fauna et Flora Fennica*, vol. 45:1 (1916) and 45:2 (1921), to have accompanied the introduction of farming.

transition from zones VII to VIII of the peat-bogs,[1] contemporary with the introduction of farming. To-day, the former occurs most freely near houses and the latter in meadows kept short by grazing or mowing, a reminder that the spread of a mixed farming economy involved more than the conversion of waste into cultivated fields.

Fortunately, in tracing the spread of farming by ecological methods we are not dependent alone on evidence for forest clearance, nor yet on that of weeds. Even more informative is the evidence of the cultivated crops themselves. For many years our knowledge of these depended on the identification of carbonized ears or even spikelets of cereals, together with the seeds of pulse crops. Under the conditions met with round the margins of the Alpine lakes much can be learnt in this manner, as the classic publications of O. Heer and later writers have taught us.[2] As a rule, however, the occurrence of carbonized material is too sporadic to give us a reliable picture of the crops grown in ancient times. Of far wider application is the technique of identifying the impressions of cereal grains so commonly met with on the sherds of hand-made pottery, which almost invariably occur on sites occupied by prehistoric farming communities.[3] The beauty of this method, perfected during the last few years at Copenhagen,[4] is that in so far as it is applied to local wares, it is capable of revealing the proportions in which the various crops were grown at specific times and places. Obviously, it only needs sufficiently numerous and widespread identifications to give us a comprehensive picture of the fluctuations and regional variations in the crops grown in prehistoric Europe.

The most recent method of studying prehistoric cereals lies in the analysis of their fossil pollen. To judge from Iversen's account[5] the limitations and possibilities of the method are as yet imperfectly defined and we must be careful not to repose uncritical trust in investigations still at an early stage of development. Yet it is not too early to see that the application of pollen-analysis may go far to advance our knowledge both of the role of cereal cultivation in general and of the relative importance of the various grains. As early as 1929, von Post first suggested that the frequencies of cereal pollen might be used to define successive expansions

[1] The zones generally distinguished by Danish students of forest development are as follows :

| *Forest Zones* | *Blytt-Sernander periods* | *Prehistoric stages* |
|---|---|---|
| IX | Sub-Atlantic | Early Iron Age |
| VIII | " Sub-boreal " | Bronze Age |
| | | Neolithic |
| VII | Atlantic | |
| V-VI | Boreal | Mesolithic |
| IV | Pre-boreal | |
| I-III | Late Glacial | Upper Palaeolithic |

[2] O. Heer, *Die Pflanzen der Pfahlbauten*. Zurich, 1866. English extracts from this work are printed in Dr. Ferdinand Keller's *The Lake Dwellings of Switzerland and other parts of Europe*. London, 1866. pp. 336-354.

[3] Any hand-made clay fictiles are liable to show grain impressions. The importance of preserving all such, as well as sherds, from prehistoric sites, for examination will be evident. This does not of course apply to wheel-turned wares.

[4] H. Halbœk, " Studies on Prehistoric and Anglo Saxon Cultivated Plants in England ", *Proc. Prehist. Soc.*, 1940, 176-178. Cf *Antiquity*, 1941, 199-200.

[5] J. Iversen, op. cit., pp. 39, 48-49.

of the cultivated area, but not until 1937 did Firbas first show that it was practicable within a small margin of error to distinguish the pollen of cultivated from that of wild grasses. As a rule the pollen of cultivated cereals is larger, has a thicker wall and possesses a distinct structure, having a large pore with a well-marked ring. On the other hand it may be noted that the pollen of barley and of *Triticum monococcum* could be mistaken for that of certain wild grasses, while certain of the latter produce pollen approaching that of cereals in size. A certain margin of error is therefore likely and this should make us careful to avoid inferring cereal cultivation from a few stray identifications. Again, it must be remembered that the relative proportions in which the pollens of the various cereals occur gives no true index of their cultivation. Neither of the cereals grown in Denmark during Neolithic times, wheat and barley, liberated much pollen : equally modest from this point of view were the cereals grown during the Bronze Age ; cereal pollen, did not, indeed, become at all abundant until the appearance of the more prolific rye. Nevertheless, pollen-analysis may one day unfold the story of the spread and early development of farming culture in any region where the method is capable of being applied. Although no precise estimate can yet be formed, the results of studying the cereal pollen in post-glacial deposits have lent support to the view that, prior to the Iron Age in Denmark, corn-growing was carried on only to a rather limited extent.

It must be obvious that, in trying to arrive at conclusions about the economic activities of early man by palaeobotanical means, we must be careful not to place undue reliance on any single thread of evidence. The clearance of forest, the cultivation of cereals and the growth of weeds in meadows, as well as on cultivated land, are interrelated processes. This has been illustrated very clearly by Iversen's investigations in the Danish bogs. By studying bog-profiles close to known sites of early settlement, such as the " Passage-grave " settlement at Troldebjerg on Langeland Island,[1] Iversen has been able to relate his botanical sequences to cultural development by tracing archaeological " scatter " in contiguous natural deposits in the manner demonstrated in 1932 and following years by the Fenland Research Committee.[2] Again and again he has found settlement by Neolithic man to be marked by a similar conjunction of ecological changes, pointing in common to the profound effects of farming on the world of plants and trees. This is well exemplified in the diagram, a section of which is summarized by fig. 1.

How far the forest was cleared during prehistoric times is a controversial question. The conclusion reached by Sir Cyril Fox[3] after collating the distribution of archaeological finds with the drift geology of the Cambridge region, that the heavy clay soils were left initially unsettled, had won general acceptance among British prehistorians. But this does not by any means exclude the clearance on an extensive scale of forests growing on lighter soils. The extent to which these

[1] J. Winther, *Troldebjerg, En Bymaessig bebyggelse fra Danmarks Yngre Stenalder*. Rudkøbing, 1935. See also Iversen, op. cit., pp. 57-58.

[2] *Ant. J.*, XIII (1933), 266-296 ; XV (1935), 284-319.

[3] *Archaeology of the Cambridge Region*, Cambridge, 1923, pp. 9, 62, 116, 224-225, and 313-315.

supported forests is itself open to debate, but perhaps it is needless to emphasize that the " open " character of the chalk and oolite of lowland Britain, for example, is man-made. It has been argued by Gradmann that during the Sub-boreal period to the early part of which the megalithic settlement of Denmark belongs, forests were extensively replaced by heaths. On the other hand, the majority of recent authorities[1] discount this theory and emphasize the widespread character of the European forest during prehistoric times. Even though the early settlers colonized soils where the forest was least dense, it would have been necessary for them to have undertaken some clearance. Given the widespread character of the forest in early times, the clearance of forest and scrub must have been an essential preliminary to farming, whether for raising crops or for improving pasturage.[2]

Now, it may be asked, if the neolithic colonization involved clearance, how was this effected ? The principal methods must have been felling and burning, often in combination. As for the felling, Dr. Jacob Friesen's experiments may be cited : using a chipped flint axe he required seven minutes to fell a fir tree 17 cms. in diameter, but with a polished axe he succeeded in five minutes.[3] For tougher woods he would have needed correspondingly longer, but it is evident that the task of felling trees of small girth was by no means insuperable. Of particular interest is the superiority of the polished axe, which may not after all be so bad a symbol of neolithic man. If forest clearance was fundamental to their economy, it is easy to see why our neolithic settlers were so concerned to obtain the choicest material for their axes ; the flint-mines and the widespread trade in stone axes acquire a new significance—and in this connection, is it not significant that the forest-dwelling Peterborough people (Neolithic " B "), were most forward in opening up the Graig Llwyd trade ? That polished stone axes were used for forest clearance in antiquity seems to be proved by the discovery of numerous scattered examples during modern forest clearance. M. Grenier[4] quotes the case of the numerous axes found after recent clearance round the spring of Bonnefontaine in Alsace-Lorraine ; evidently the spring attracted settlers who proceeded to clear the forest and cultivate the soil within a convenient radius.

Yet it is likely that burning played a predominant role in early clearance. It needed less effort, requiring only a certain amount of preparation, such as ringing bark—an easy task for the axe—to assist in drying trees in preparation for the fire ; and it dovetailed excellently into the extensive or temporary occupation of a land characteristic of the neolithic colonization of northern Europe. Under a system, which involved the frequent breaking up of new ground and the abandonment of plots after a few years' cultivation, it would have been uneconomic

[1] e.g. J. Iversen, op. cit. 43.

[2] The present writer has since discussed the problem of forest clearance in neolithic Europe at greater length in " Farmers and forests in neolithic Europe ", *Antiquity*, June, 1945.

[3] Cited by H. Nietsch, *Wald und siedlung im Vorgeschichtliche Mitteleuropa*. Mannus Biblio. no. 64. Leipzig, 1939, p. 70.

[4] A. Grenier, " Aux origines de l'economie rurale," *Annales d'histoire economique et sociale*, 1.2e, 1930. p. 29.

to clear the land systematically and permanently. What we have to envisage is the burning of undergrowth and the felling of the smaller carbonized trees. Many of the larger tree-trunks might have been left undisturbed, not to mention the roots.[1] It must be remembered, also, that the neolithic farmers cultivated the land with hoes, and that even with the plough a certain number of tree-trunks would have constituted no serious obstacle. The neolithic farmers, in fact, cultivated patches in the charred and partially cleared forest, benefiting from the potash in the ashes from the burning.

Such a system has been followed up till modern times in the forests of Finland,[2] Carelia and the Carpathians, and survived in France[3] up till quite recently. On *a priori* grounds it is likely to have obtained in prehistoric Europe. It is not improbable then, that the signs of burning found at neolithic levels in the Danish bogs may relate in part to clearance for cultivation of the soil. That the pollen of cultivated cereals appears immediately above the level of the burning certainly argues in favour of this. On the other hand, farming in northern Europe has always included stock-raising, as well as the cultivation of crops, and as we have seen, this element was probably of special importance in the earliest stages. The appearance of the plantain is to be linked with the extension of grazing, and it is probable that the burning of forest may have been connected as much with this as with cultivation. Moreover, it must be remembered that up till modern times, the forest was itself the most important of grazing grounds. Most probably one of the main objects of the burning was the stimulus it afforded to the growth of herbs, the mainstay of early cattle-feeding. Thus it will be seen how the various features of the neolithic levels in the Danish bog-profiles—the evidence of burning, decline of forest trees and corresponding increase in herbs, accompanied by the appearance of plantain and cereal pollen and followed by regeneration of the forest, in which the birch became temporarily more prominent than the slower-moving oak—all combine to testify to one fact of predominating importance, the nearby presence of a farming community. The clearance of forest, the cultivation of the soil and the fostering of pasturage are rhythms of human activity, the activity basic to the whole subsequent development of European civilization. From the neolithic settlement onwards, the oscillations in the pollen-diagrams are no longer mere registers of geochronological data ; they are veritable documents of human endeavour.

[1] In the Dalevala (Runa XXI, 350) the aged Viänämoinen says of his voice that it halts

" Like the hoe among the pine-roots."

Everyman edition, vol. I, p. 248 (bottom).

[2] cf. G. Retzius, *Finnland, Schilderungen aus seiner Natur, seiner alten Kultur und seinen heutigen Volksleben.* Berlin, 1885, pp. 42-45 ; also I. Manninen, *Die Finnisch-Ugrischen Völker.* Leipzig, 1932, pp. 28-30, 245 and 274-275.

[3] cf. Marc Bloch, *Les Caractères Originaux de l'histoire rurale francaise.* Oslo, 1931, p, 21.

## II. THE NEOLITHIC PEOPLES OF SOUTH-WESTERN EUROPE

*Speaker :* Mrs. C. F. C. Hawkes, M.A., F.S.A.

My title is the Neolithic Peoples of South-Western Europe, but it is clearly impossible to consider this subject without extending it in space to cover the whole of the Atlantic coast regions, and in time to include Chalcolithic and even later cultures. Within this field, there are three main cultural elements which seem to demand consideration and to have some distinct identity, often though they impinge upon one another. These are (1) the cultural element represented by special forms of incised ware, (2) the contrasting plain ware element which includes the familiar Western Neolithic and (3) megaliths in all their fearful complexity.

For my part in any discussion such as this in which the emphasis is laid on " problems ", I should give the first of these, the incised-ware cultures of the Mediterranean or beyond, a very high place. They have long seemed to me of absolutely fundamental importance both for the understanding of early spreads of " neolithic " culture from the East to the western Mediterranean and then beyond that from the Mediterranean to West and North Europe. These cultures manifest two main streams, undoubtedly, however, interconnected. One is North African and Spanish : we know it from cave sites in Morocco—and Dr. Coon's recent discoveries there may reveal much more—from whence it spread across the Straits to produce the South Spanish Cave culture. It appears that the Moroccan culture looks back, somewhat remotely, to Egypt. The other stream is that which links the inadequately explored earliest neolithic of Crete with the east Sicilian and south Italian material of the Stentinello group and then goes on perhaps jointly by way of Liguria and of Sardinia, to southern France where it diffuses throughout the country as the Chassey culture. Malta also comes into the picture and encourages us to look for Near Eastern origins in this whole group of cultures. The main problems, then, which come to my mind beyond the basic one of the origin and racial significance of the incised-ware cultures, are, first, the relationship of the two main streams I have described, and, secondly, and more difficult, the nature of the undoubted connection existing between the Cretan-Sicilian-south French stream with the Balkan culture represented at Butmir (Bosnia) which in turn certainly links up with Danubian affairs. Two lesser questions of interest are the origin of the channelled ware element, which constantly associates itself with the incised-ware cultures yet appears to remain essentially independent, and, second, a racial question. The people of the S. Spanish Cave culture are generally supposed to have provided the main part of the Beaker people. These Beaker people when they reached Britain and elsewhere were notoriously roundheaded. This brachycephaly cannot have come from the Corded Ware people. Did it therefore come from Spain and can it be adequately traced among the South Spanish incised-ware stocks ? Hitherto I believe only the faintest hints of it have been discovered.

I shall now pass on to my second principal group, that of the plain ware. Western Neolithic cultures. This, at the moment, seems to present fewer difficulties, and I shall therefore say less about it, despite its great interest for British archaeology, It is not, I think, disputed that it was first implanted in our region as the Almerian culture of south-east Spain, probably as soon as 2700 B.C. and represents the earliest appearance of a settled neolithic society with an advanced agriculture. Nor has it been questioned that the people who brought the Almerian of El Garcel type inherited something from the neolithic Egyptians of Merimde and Fayum. The problem, however, which is always encountered here is the need to bridge the chronological gap between the Egyptian cultures which even at Fayum cannot have lasted much later than 3700 and the settlement of Almeria at least a thousand years later. One has to say that the culture had a long life somewhere in the peripheral regions of Egyptian civilization—it would be more satisfactory to have evidence of it.

There is still very general agreement when we pass on to the main outline of the spread of this culture northward and westward from Iberia. By 2500 it was spreading as far as Switzerland and the Rhineland on the one hand, and within our territory to north-west France and southern England ; it is, I think, less clear whether the north-west French settlement, and particularly that of Brittany, took place entirely from landward, or whether it was augmented by our coastal route. Another important point of agreement is that in all these regions this culture was established before the introduction of true megalith-building—that there is a definite pre-megalithic Western Neolithic phase. I would say here in passing that it would be interesting and valuable to see if such a pre-megalith phase could be traced in Portugal. Going on one step further in time we are still, I hope, secure when recognizing in most of the regions under consideration, Iberia, western France and Britain, including now the Atlantic coasts of Britain, an early megalithic phase which was definitely pre-beaker. There is, however, a question here which has not perhaps been sufficiently examined. How far is the plain pottery and other remains from our British highland zone megaliths to be attributed to established Western Neolithic Windmill Hill people spreading up ultimately from the early settlements in southern and eastern England, how far to the megalith-building newcomers who would bring related western neolithic traits from Iberia or France ?

I come now to my last main cultural element, that of the megaliths. The various questions as to the lines of megalithic diffusion and the inter-relationship and internal histories of the distinct megalithic regions were the subject of so much controversy just before and at the beginning of the war, that in any discussion of outstanding problems it is necessary to give more time to them than their true historical importance would seem to warrant. The chief protagonists were Professor Daryll Forde and Dr. Daniel, and the controversy arose as a result of the view first, I think, comprehensively stated by Professor Childe, that in the megalithic spread from the Mediterranean and Iberia to France, Britain and the

north two main streams may often be detected, that characterized by gallery graves with chambers parallel-sided and all of a piece and that characterized by passage graves with narrow passage leading into a distinct chamber. That this distinction can be both valid and valuable when sensibly applied is, I think, undeniable, but to be sensible it is necessary to allow for a large number of complicating factors. It must be remembered that in their Mediterranean origins these two forms share a common ancestry, and have therefore common genes which may at any time emerge; that even in regions where the two types did clearly differentiate themselves there was always a certainty of mutual influence and interchange of traits, and above all that there was a frequently repeated tendency for passage graves to degenerate into apparent gallery graves and for either to degenerate into nondescript cists. Therefore while the dual classification may be most useful in following a continuous line of cultural diffusion or for comparing the tomb practice of adjacent regions, it is absolute madness to apply it to far-flung regions with no other cultural support and the most inadequate of chronological guides. Indeed the study of megalith typology in isolation from other cultural factors is far worse than useless except within small compact groups. This is the core of the Forde-Daniel controversy, and although in general I am not going into details, it is essential to expand a little at this point. Daniel claimed that the Cotswold-Severn megalithic long-barrows were a special variety of the true gallery with side chambers added—he devised the name of transepted gallery grave—that they were to be derived from southern Brittany and the Lower Loire and thence linked with spread of galleries overland from Mediterranean France. Professor Forde agrees with the connection of the British group with those of south Brittany, which indeed he himself was the first to clarify, but insists that both the French and British groups are forms of side-chambered passage grave, rather late in passage grave history. I do not think it is possible to answer Forde's destructive criticism of the unreliable basis of Daniel's typological handling of the French tombs; and I personally agree with Forde also that the predominant element in the Cotswold-Severn group is the passage grave. Again in passing may I say that I think in circumstances where passage graves and galleries may legitimately be distinguished, far more attention should be given to the side elevation of the tombs compared. The truly differentiated gallery or long cist should be of even height and solidity from end to end, whereas the passage grave and its derivatives show an increase in height and massiveness of stone from the entrance towards the inner end. Our south-western long barrows, where it is possible to study this characteristic, tend to show such an increase. It seems that the main conclusion to be drawn from the controversy is the folly of rigid, isolated typological studies of megalithic tomb plans. For heaven's sake do not let us think that this urge to tie all the works of man into neat classification with neat labels is "scientific"—I believe it will make our subject look not scientific but ridiculous.

Enough on this point, but there remain two other issues opened for further debate by Dr. Daniel which cannot be ignored in any study of the spread of early

civilization from south-western Europe. One is his suggestion that the route by which megalith-builders reached northern Europe was not, as all recent writers have maintained, by the Atlantic coasts and northern Scotland, but already by an English Channel and North Sea route. I should like to hear comment on this. The other question concerns the social basis of this megalithic diffusion. Daniel quarrels with those who refer to megalith builders as missionaries or small groups of leaders exercising a religious domination, and prefers to call them colonists. The question of the numbers of persons involved is of course significant, and is in fact really the same as the point I made just now—as to whether the culture represented in the British tombs is mainly native Windmill Hill or a fresh continental influx of Western Neolithic. But the distinction between religious leaders and plain colonists seems to me meaningless, and leads Daniel to the surely absurd statement that " Burial chambers are essentially not religious but funerary monuments " : to say that burial chambers are funerary monuments seems hardly necessary, to say that they are not religious seems extraordinary. This may look rather like a tangent to my main subject ; perhaps it is, but it is going to lead me gracefully on to my close. I should wish to end by saying that the great importance of south-western Europe in the period assigned to me is for the vital question of the spread of civilization from east to west and north, and in studying this I would urge that the religious aspect is not forgotten. I believe that fascinating work waits to be done on the connections between early historic religions in the Mediterranean and the prehistoric religious manifestations in the west and north. However, I do not want to over-emphasize the religious aspect—it is only right to single it out at all because it has been so much neglected—what I do want to do is to beg for much closer collaboration between those who work on Near Eastern and Mediterranean civilizations and those workers who concentrate on regions nearer home. When they do so, their readiest meeting place must inevitably be south-western Europe.

## Discussion

*Professor Daryll Forde.* I would like to say a few words on what both Mrs. Hawkes and Dr. Clark have said about methods of archaeological study. There is a serious tendency in the archaeological world to-day to a specialism, which is resulting in narrowness of outlook. This afternoon it has been shown clearly how archaeology must link up both with anthropology and with the natural sciences. Here I think we have the core of our archaeological approach, the need to relate all our field material to findings in other branches of knowledge in order to understand the activities of prehistoric man. Every question of cause and effect establishes links not only with anthropology and ethnology but with many branches of the natural sciences on the other.

*Mr. Grimes.* I should like, if I may, to speak as an excavator rather than with any special authority. I would like to emphasize the danger of the archaeologist who concentrates on things rather than on the people who made and used them, and the conditions in which they were used. There is no greater danger when Neolithic problems are considered than this subject of megaliths. I should like to see a scheme for the exploration of a series of selected megaliths throughout the area of megalithic construction by really

modern methods. If this could be done, I think we should go a long way towards obtaining the answer to a number of questions which now concern us, for instance, the relationship of different types of pottery to different kinds of megalithic structures. I think that the time is now ripe for some such scheme.

*Mr. Miles Burkitt.* We have listened to two very interesting papers. I am not going to spend any time discussing any of the many neolithic problems raised. But some veiled attacks have been made on the use of typology and technology as the foundation for any training of a student interested in prehistory. New recruits will be coming forward, we hope, after the war, and they will have to start from the beginning. I do hope they will be given a sound grounding in the grammar of the subject. We all want to help to clothe the dry bones of prehistory with flesh and blood. But this can only be done safely by those who have been taught how to fit the bones together !

*Dr. C. F. A. Schaeffer.* We must keep a sense of proportion when we are considering megalithic monuments. In the Neolithic and early Bronze Age, periods when domestic architecture is so insignificant and religious architecture so striking, we tend to pay too much attention to the religious side. For example, in Brittany, with its impressive monuments, we might say the people were very religious and perhaps daily churchgoers, but that would be because our evidence is incomplete. It is thus the accident of preservation of monuments which gives us wrong impressions of the occupations of the people of the period. If the City of London were completely destroyed, including St. Paul's, and the remains later excavated, we might think of it as a city of churchgoers rather than a city of bankers.

*Dr. S. Huzayyin.* The question of the connections between cultures of the eastern and western Mediterranean is a very interesting one. In studies of the eastern Mediterranean, too much attention has been paid to expansion by sea rather than coastwise. Recently, more work has been done in the Sahara, and an effort has been made to link up with eastern Spanish tombs and paintings. Work here may do something to fill the gap of a thousand years between the Neolithic cultures of Egypt and those of Spain.

*Mrs. Baumgärtel.* North Africa may well have played an important part in the spread of culture. Mrs. Hawkes spoke of the religious implications of certain aspects in Europe, and the position in Egypt is very similar.

*Chairman's Remarks.* Before asking you to express your gratitude to Dr. Clark and Mrs. Hawkes, I want to make one important remark, and that is about how strongly I agree with Mrs. Hawkes in the question of the study of megalithic typology. Nothing is more misleading than such a treatment of sepulchral plans divorced from context and content, and I must apologize to the archaeological world for any steps I may have taken to mislead them. I now make a public recantation of this, which is most unmarxian of me.

# FUTURE RESEARCH ON THE BRONZE AGE IN EUROPE AND THE NEAR EAST

*Chairman :* Miss H. L. Lorimer,
*Late Lecturer in Homeric Archaeology in the University of Oxford*

*Speaker :* Dr. C. F. A. Schaeffer, M.A., D.Litt., F.S.A.,
*Curator, French National Museums; Director, Ras Shamra Expedition*

Towards the end of the Neolithic period, with its agricultural organization so favourable to the development of regional, self-supporting units, a sudden and refreshing impact of south-eastern and southern influences reached central, western and northern Europe. The advance enjoyed by the Mediterranean and Near Eastern World in social organization and metal technique gave impetus to exports from there to Neolithic Europe. Existing trade routes were developed, new ones created. A general search for mineral deposits began. Competition for their possession and exploitation started. A regrouping according to the mineralogical structure of Europe altered and in some measure simplified the complicated mosaic of regional cultural units developed during Neolithic times.

New inventions, tools and weapons generated fresh ideas and revolutionary new conditions. The novelties penetrated far and wide and where pockets of conservatism remained, they were left behind. The innovations swept on victoriously from one end of our continent to the other.

Wherever research is undertaken in European Bronze Age archaeology, the journey ends somewhere in the East or the South. Gordon Childe, applying his spadework in north Scotland, came to re-write the *Dawn of European Civilization*, but had then to write about the Ancient East, as far as India.

Christopher Hawkes, author of *The Prehistoric Foundations of Europe to the Mycenean Age*, even if he restricts his study to Bronze Age pins from a Ramsgate find, has to invite his readers to travel with him over to Picardy, then to my beloved Alsatian tumuli, and as far east as Bohemia. Unpublished finds prolong the journey now to Syria and the Near East.

These connections between Orient and Occident and Occident and Orient, for the interchange was not, of course, and could not have been, a one-way traffic, must be investigated on the broadest base possible. Those who choose to explore the Bronze Age, and it is a wonderful study indeed, must be aware of the need to take in from the start the archaeology of two continents and in addition some philology ; for during the Bronze Age the alphabet was born with the resulting spread of written knowledge. Co-operation in archaeological and historical research is now intimate and cannot be divorced. The chronology is no longer

guess-work, but a complicated precision instrument, the main elements of which are Egyptian, Assyrian, Babylonian, Syrian, Cretan and Mycenaean data. King-lists and archaeological evidence have to be brought into accord, the latter having revealed anew its possibilities when recently Babylonian records have been proved to be misleading with regard to the beginnings of the first Dynasty.

May I insist here on the fact that the lowering from round 2000 to 1750 B.C. of Hammurabi's reign *does not* start a landslide of European Bronze Age dates. The operation has been an internal affair of Near Eastern and Egyptian chronology. The average date of Bronze Age beginnings in Europe is and remains the round figure of 2000 B.C. For the most advanced regions, the *terminus ante quem* goes as far back as 2100.

*One* pillar of European Bronze Age chronology, the hoard from Byblos, with its toggle pins and ingot torques similar to those of Hungary, Bohemia, south and north Germany and eastern France, can no longer be fixed round 1800 B.C. It stands further back, certainly between 2100 and 2000, as new research shows. Besides, the famous hoard was not a gift to the temple containing samples of foreign bronze types the most conspicuous of which were alleged to come from the Caucasus. This hoard of Montet contains unfinished bronzes and wasters. Nobody took notice of this fact. It cannot be admitted that unfinished bronzes and wasters were brought over from countries as far away as the Caucasus. Hence the Byblos bronzes were not imported from there.

Moreover at Ras Shamra we found the necropolis of people who have worn these ingot torques and toggle pins as their distinctive attire and had them in gold fixed round the neck of their crude figures of male and female gods. Tombs of this kind are known also from Karkemish, Kalaat er Rouss and Hama in the Orontes Valley. Why should all these people living *en masse* in Syria between 2100 and 2000 have worn Caucasian ornaments ?

Finally, Lucas, the technician from the Cairo Museum, and Wainwright have shown that in the neighbourhood of Byblos copper as well as tin ores are washed down from the Kerouan mountains and can be picked up in the river beds. So all conditions for the development of a bronze industry were available on the Syrian shores, which renders even more improbable the hypothesis of Caucasian imports.

But had the Caucasian or south Russian bronze-smiths ever produced ingot torques and toggle pins which would support the idea of export from there ? As far as the torques are concerned, everybody repeated that they had been found in the Caucasus and Chantre's neat drawings of them were quoted and quoted again. Nobody seemed to worry that most of these torques were too small to fit a woman's neck. Also, it is clear from Chantre's Caucasian report that he didn't find any of these torques himself, but bought them from professional tomb robbers. He explains them as torques with reference to twisted torques from Hallstatt tombs in southern Germany and Switzerland. The only archaeologist who has found those so-called Caucasian torques *in situ* was Bayern, a German amateur digger

living in Tiflis. He published a careful account in his booklet *Gräber und Schatzfunde*. It is clear from his observations that the Caucasian torques were not torques at all but what Bayern terms " Kopfringe ", fixed always by pairs at each side of the women's hairdress. The oxide had coloured both temporal bones of the skulls.

Therefore, the Caucasian Bronze Age torques only exist in the archaeological literature. I am afraid that they will remain there.

We are nevertheless obliged to investigate the reputation the Caucasus enjoys as an early metal working and export centre with clients in central Europe and as far to the south as Sumer, according to several authors. It is clear that Gordon Childe's early offensive against that view in his Aryan study was absolutely on the right line. New Russian research and also our own, when they are published after the war, will show that the Caucasus was a very conservative province of the eastern Bronze Age world. It did not play an important role in the general development.

For Maikop and several other early Bronze or Copper Age finds from south Russia, we have now more definite dating to propose : they are to be attributed to the period between 2200 and 2000, not near 3000 as Schmidt and Rostovtzeff first thought and not as late as is claimed by Tallgren, who puts them down to between 1600 and 1500 B.C., misled as he is by Nils Aberg's new, extra short Bronze Age chronology.

Let us here pass clear and unmistakable sentence against this Nordic attempt to equate the site of the second town of Troy with that of the shaft graves of Mycenae and so to disjoint the whole structure of European Bronze Age chronology. There are enough uncertainties to be cleared away, that we do not need to obstruct research with wrong classifications. Here, then, is a field where with the aid of already existing material it is possible to advance to sounder results than those we have at present achieved.

I do *not* propose the abolishment of scores of Bronze Age chronologies and classifications worked out in various countries all over Europe. There is something in maintaining these different schemes for the ordering of the material found in the respective countries. But there is certainly need of a general Bronze Age chronology for which I would propose to maintain the old tripartite division : Old, Middle and Recent Bronze Age, with for each one of these periods three sub-divisions called a, b, c, or better, 1 to 3. That would give us nine divisions for a period lasting round a thousand years, a sufficiently ambitious scheme for the next generation or two.

There have been many praiseworthy attempts to increase the number of sub-divisions, but all classification is conventional and to simplify means to remain more in accord with the facts.

Moreover a tripartite classification has the advantage of facilitating the equation with the Bronze Age data of the Mediterranean, the Near East and Egypt. There, the general division into :

| | | | | |
|---|---|---|---|---|
| Old Bronze Age | ending | round | about | 2100 B.C. |
| Middle | ,, | ,, | ,, | 1600 B.C. |
| Late | ,, | ,, | ,, | 1200 B.C. |

with, in each case, three sub-periods, is securely established. I shall come out with such a scheme after the war and hope Gordon Childe, Grahame Clark, Christopher Hawkes and other specialists will help to achieve some measure of general agreement.

Another aspect of Bronze Age research which in my view needs development, is the use of the purely scientific devices, such as pollen-analysis, combined with observation on post-glacial variations of climate and their evidence in the lakes of Switzerland, the moors of Scandinavia and north Germany, the changing altitude of Bronze Age dwellings in the Alps and other mountainous countries, or the distribution of grassland and forests in central Europe.

It is true, as we were recently reminded in a delightful Pelican edition of *Prehistoric Britain*, that the facts deduced from this evidence cannot be too firmly tied to fixed dates. But apart from their help for chronological purposes, they are extremely informative with regard to many aspects of the Bronze Age civilization as a whole.

Analyses of Bronze Age metal and other material could be multiplied tenfold with the greatest advantage. We should produce the material for the specialists even if that means punching some holes in our beloved bronze artifacts. There are many publications of important Bronze Age sites with beautifully coloured plates of metal types and painted pottery costing hundreds of pounds. But no money was spent on analyses, of which usually none are given or only one or two.

Tools or weapons are too often identified as being of copper or bronze according to the aspect of their patina. But even if mistakes could be avoided, we need to know much more about the composition of the bronzes : especially whether they are natural or spontaneous bronze or an intentional alloy, what the impurities of the original ores were and so forth.

With such indications the question of origin and date can often be fixed with certainty, mostly with great probability and our research is at once more securely directed. More analyses mean more light, provided that they are sufficient in number and the results be considered as an average. Isolated analyses may be misleading as they may refer to exceptions. Also, the result of an analysis is one thing, the interpretation another.

This can be demonstrated with regard to the Baltic amber. According to the current manuals, the production and distribution of this not very essential stuff has brought about whole trade routes right across Europe to the Aegean and even farther to the Near East. Baltic amber was honoured as a potent instrument for the diffusion of Bronze Age culture. I have only time to state here that the fame which Baltic amber derived from the discovery in the Kings' tombs at Mycenae and from other important finds is somewhat fragile, because Nordic amber, often

improperly called Prussian or Baltic amber, is found along the coast from east Poland to Holland ; its southern limits, already known to reach as far as Saxony, are not yet ascertained. Moreover, the presence of amber acid is not an absolute criterion for the Nordic origin of this fossil-resin, as already admitted by Helm at the end of last century.

Whenever amber is found in Bronze Age sites in Europe and as far south-east as Persia, it is invariably considered to be Baltic amber, and the conclusions are somewhat romantically coloured. Moreover, several analyses have shown that south European and specially the Mediterranean amber was widely known and used during the Bronze Age and that amber beads of this period found in Switzerland and south-central Europe were *not* of Baltic origin. It is perhaps unfair to stir up doubts against solidly accepted views, especially when the void thus left cannot be filled with positive argument, but as long as we deal with inadequate material and scanty information the constant application of doubt is a good safeguard in our research.

Clearly, I should now state the blanks in our knowledge of the European Bronze Age which would need most urgently to be filled. Well, I am afraid we have here really *l'embarras du choix*. In general the initial periods of the European Bronze Age, and then its closing stages are the most obscure. We have no idea yet of the relative importance and chronological relationship of the various currents which, at the beginning of that age, drove north, then west and east. The remarkable migrations of the so-called Beaker Folk, for example, from Spain to Britain, Scotland and Ireland, also to the western Mediterranean, east France, north Italy, and their invasion of central Europe, is full of unsolved problems. One must now add the question of the relationship of these people to the beaker users who, at the same time or just before 1900 B.C., made their appearance as intruders in the southern lands nearest to Europe. I am bound to say that compared with the systematic research carried on in this country, Scandinavia and Germany before the war on this and other Bronze Age problems, our research in France was not sufficiently co-ordinated. I hope that we shall set up a priority list of problems which need to be dealt with most urgently.

Here is another problem, the investigation of which would probably be best accomplished by team work : there was a general catastrophe in the ancient world and parts of protohistoric Europe which, towards 2300, broke down many of the Copper Age and Old Bronze Age cultures. The crumbling of the Ancient Empire in Egypt was only one of its episodes. How it came about and why, we really need to know. At present we can follow the ruins and disturbances from the Caucasus to western Europe and, on the Asiatic side, from south Russia and Persia to Syria and the valley of the Nile.

The Urnfield peoples' invasion from about 1400 B.C. onwards was another great impact upon our Bronze Age world. Much work has been done on the subject in this country, in Switzerland and some in France ; but we are still confronted with more question marks than ever. The Urnfield culture recently

revealed in Syria and which also became evident there towards 1400 B.C. has not yet been investigated for our benefit.

Really, such extensive investigation throughout vast regions divided now by political frontiers guarded by passports and customs officials, should be undertaken in co-operation by archaeologists of the various countries concerned. There is need, once more, for planning our research on broad lines.

## Discussion

*Professor V. Gordon Childe.* Dr. Schaeffer's brilliant paper has given us a much clearer picture of the Bronze Age. The illustrations he has given us are surely confirmation of the view I put before you yesterday, that archaeological objects are facts, which false interpretation cannot completely obscure. The bronze torques are solid pieces of metal, and, even when they themselves are not available, there are good drawings, and from the drawings we are able to deduce their true character. With their aid we are able to prick the inflated bubble of false deductions based on them.

One point that I would like to emphasize is that in cases where a lot of scrap metal is found, it represents wealth as such, regardless of the actual objects included. This point must be remembered in considering hoards. I wish also that Dr. Schaeffer had had time to tell us something of his views on the theory put forward by the German, Witter, that central Germany was a metal-producing centre in the Bronze Age.

As regards amber ornaments, one must remember that deductions as to their source depend as much on form and structure as on the material, and that this must be considered in any evaluation of sources of supply and trade routes.

I am in agreement with Dr. Schaeffer in his acceptance of the tripartite division of the Bronze Age. In Western Europe, however, one must realize that one cannot depend on typology of weapons, etc., alone, since they are not found everywhere, and we are left with the conclusion that in some areas there was no Middle Bronze Age at all !

*Mr. Miles Burkitt.* I should like to ask Dr. Schaeffer what he considers is the date of the Tell Halaf level at Mersin ? Below it was found a chalcolithic level containing sun-dried brick, ashlar masonry, and painted pottery. Under this again was a thick stratum of burnt ash which covered a series of neolithic layers. All these contained small green polished celts, tools made of chert and obsidian and undecorated pottery. Towards the base, however, there was a level which yielded incised pottery of a special type which, I believe, has been found unstratified at the bottom of the dig at Ras Shamra. Now exactly similar green polished celts—made of rock foreign to the district—occur in material from the Swiss Lake villages. But how can the chronology be made to fit ? The importance of an exact correlation between the chronology of the Near East and western Europe is obvious, but this has not yet been satisfactorily accomplished.

*Dr. Schaeffer's reply.* As regards periods, my classification is intended to be chronological and not typological.

On the subject of amber, the point I wished to make is that it has been given undue emphasis, but I did not mean to suggest that there is no northern amber in the Mediterranean. The fact that there is amber found in the Mediterranean makes it probable that the people there used it and this is proved by analysis. The proportion of Baltic amber in the Near East is unknown. Amber was not essential enough to influence trade routes.

With regard to the date of the so-called Tell Halaf pottery, I cannot give a definite answer since at Ras Shamra I have only reached this level in a *sondage*. I am sure, however, that it is considerably earlier than the Jemdet Nasr period. The thickness of

the strata can only give us an approximate date, but from this I should say that it must go back to about 3,000 B.C.

*Dr. F. E. Zeuner.* As regards amber as an object for trade, it should be noted that the East Prussian variety is found, as a result of the glaciations, as far west as East Anglia and Holland, and south-east to central Russia and the Ukraine. Individual finds of amber, therefore, do not prove amber trade, though it must be admitted that only East Prussia can have provided quantities sufficient to keep a trade going on an appreciable scale. Another important source of amber is Sicily, and *a priori* one would expect that Mediterranean peoples were acquainted with Sicilian amber rather than the East Prussian variety. I do not know whether the provenance of prehistoric amber specimens found in Greece and the Near East has been investigated. Analysis would reveal it without undue interference with the specimens.

Dr. Schaeffer mentioned the climatic problems in the Bronze Age and their economic importance. It appears that in the course of the Bronze Age a change towards drier conditions was going on. But unlike other Postglacial changes of climate, this one cannot yet be explained climatologically, and the evidence even is regarded as inconclusive by some workers. I would like, therefore, to ask excavators of Bronze Age sites, when they find good sections containing Neolithic followed by Bronze Age and overlain by Iron Age, to call in an expert to study the soils.

*Mr. C. F. C. Hawkes.* Dr. Schaeffer has shown us that times have indeed changed at the Musée de St. Germain since Salomon Reinach sat there and wrote the *Mirage Oriental.* In fact I think he will himself welcome a reminder, that the Bronze Age cultures of Europe were no mere pale reflections of the Light from the East. We may call the problem one of diffusion ; but it is also one of action and reaction. For example, Dr. Schaeffer's demonstration of the Syrian origin of the Byblos hoard does not at all upset the idea of contemporary schools of metallurgy in Europe. The Orient doubtless provided prospectors and entrepreneurs, technical skill, and types to copy. But the European industries which such schools created, using native European ores, soon transcended their Oriental inspiration, and took on more and more each a character of its own. This illustrates the basic historical importance of the Bronze Age of our Continent, for which no emphasis can be too strong : it was then, once and for all, that Europe became European. Traditions, both of material and of social and political culture, were then established, which run on through all later European history.

A sound chronology for the period is of course then all the more important, since it is necessary to show the rate and above all the direction of diffusion. It is here that we must particularly welcome Dr. Schaeffer's work. We need it now more than ever, because of the false notions of chronology which have been used to enforce the German claim of superiority in nature, and therefore of priority in time, for the " Aryan " achievement of civilization. As a signal example of such, the more regrettable for the accompanying array of really valuable technological data, I would quote Witter's *Die älteste Erzgewinnung im Nordisch-Germanischen Lebenskreis* (2 vols., Mannusbücherei, 1938). And if, as we must, we are to replace all the Kossinna-Teutonic nonsense by a just estimate of the action and reaction between the Orient and Europe in the Bronze Age, we should let Dr. Schaeffer's lead inspire us to work on these matters internationally.

Lastly, Near Eastern archaeologists should remember that in the later Bronze Age at any rate, Europe had something to give to the Near East. The Urnfield or " Lausitz " irruption round the North Aegean about 1200 is now well known. Whence then came the Urnfield culture which appears about 1400 in Syria ? The whole question of the famous Land- and Sea-Raiders of those troubled times is still much too obscure ; and here European archaeology may come to repay Near Eastern for its assistance in charting the centuries before.

# PROBLEMS OF THE BRONZE AGE AND THE BEGINNING OF THE EARLY IRON AGE IN EASTERN EUROPE

*Chairman :* Emeritus Professor E. H. Minns, Litt.D., F.B.A., F.S.A.

*Speaker :* Professor T. Sulimirski, Iur.D., Ph.D., Hon. F.S.A.
*Professor of Prehistoric Archaeology in the University of Cracow*

In the following lecture I am going to consider the problems of the Bronze Age and the Early Iron Age which particularly require wide co-operation between the archaeologists of Central and Eastern Europe. These problems are, however, too numerous to be discussed in the short time at my disposal. I shall therefore confine myself to certain of these problems which seem to me more important, and which at the same time I know best.

I will begin with the Black Sea countries, and with the Ukraine in particular. In speaking of the Ukraine and of the Black Sea countries, however, it should be remembered that this area falls quite clearly into two zones (fig. 1). The first, lying nearer the Black Sea, is a wide belt of true Steppe, which from remotest times was inhabited by nomadic peoples.

The second zone is formed by the area lying between the true steppe and the eastern European forest lands. It is known as Forest-Steppe. This is an unusually fertile black soil region. It was always inhabited by peaceful agricultural peoples beginning with the painted pottery culture.

Among the chief problems of the Bronze Age in this area is the group connected with the painted pottery culture. This culture obviously appears and develops chiefly in the Late Stone Age. But to-day it is already indisputable that it remained in existence in any case until about 1700 B.C., that is, up to times which in neighbouring territories are included in the Bronze Age.

There is a voluminous literature on the subject of the painted pottery culture. An enormous amount of archaeological material, only a small part of which has been published, is to be found in hundreds of museums almost everywhere in the world. The very extent of this material is probably one of the main reasons why, nevertheless, this culture has not so far been conclusively dealt with as a whole. It is also urgently necessary to consider in detail the individual types and styles which are met with in painted pottery, and to fix their extent, chronology, mutual relationship and so on.

The above problems, however, rather concern the Stone Age. On the other hand, the question of the downfall and destruction of the painted pottery culture is a problem precisely of the Bronze Age. It is almost universally agreed that this culture was destroyed by a foreign invasion about 1700 B.C., and moreover one

not infrequently meets with the statement that the bearers of this culture were completely annihilated by this invasion, or else driven out from their settlement.

However, where historical sources are available, processes of this kind are

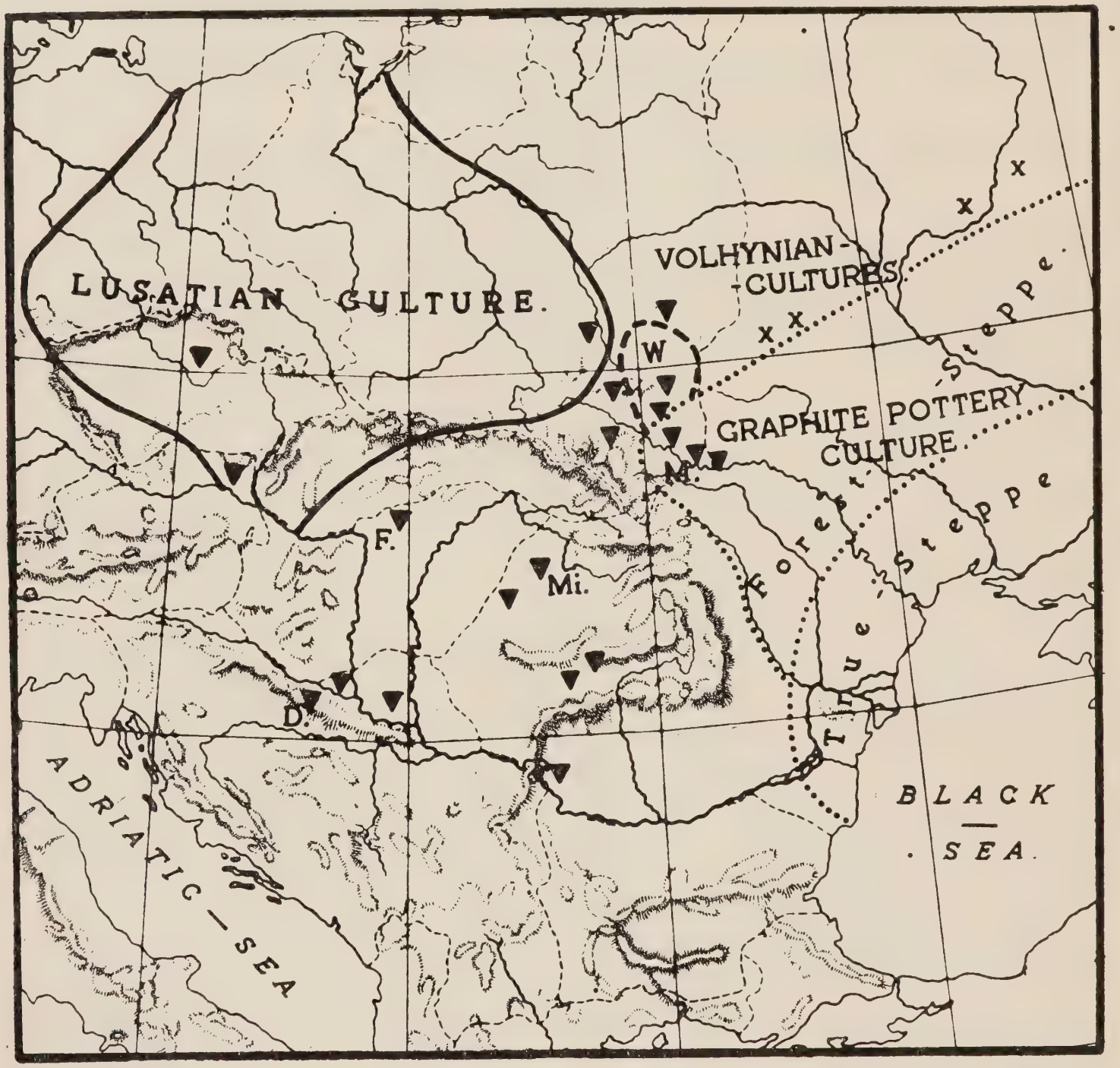


Fig. 1. Extent of the finds of Thraco-Cimmerian type and of some other contemporary cultures.
W—Wysocko Culture.
▼—Finds of the Thraco-Cimmerian type.
X—Finds of the Volhynian Cultures.

not noted, specially when such a large ethnical group is concerned as is represented by the bearers of the painted pottery culture. The disappearance of a prehistoric culture is not synonymous with the disappearance of the people it represented.

The old population, subjected to new influences and a new rule, transforms its former culture, and after some time there appears in the same area a new culture, still possessing certain elements of the former culture.

In the case of the painted pottery culture, in my opinion, we undoubtedly have to do with the transformation of an old culture, and with the continuity of the settlement, and not with a change of population. To this conclusion I have been led by the results of my observations in the area of south eastern Poland.

In about 1933, I conducted with Dr. Pasternak, the director of the Ukrainian Museum in Lwów, a few days' research in two localities of the district of Zaleszczyki, in Holihrady and Nowosiółka Kostiukowa, within the area over which the painted pottery culture extends.[1] In both localities we came upon the same type of settlement, possessing very characteristic pottery of two species. One of these kinds of pottery was coarse grained, with a rough brown surface, and was known to be from the late Bronze Age. The other type was beautifully finished, with a black graphite surface, with channelled ornaments. This black graphite pottery was so similar to the graphite pottery known from Scythian graves, and also was so closely related in character to the Lusatian culture pottery of the Hallstatt period that I dated it at first within the Hallstatt period.

In both settlements we remarked that among the black graphite pottery, specially in the lower levels excavated, a considerable quantity of typical painted pottery appeared. I did not at that time attach any greater importance to this fact.

However, even at that time, the fact struck me that both types of pottery, that is, both the black and the late painted pottery so much older than the former, as I then supposed, should be present at the same level in the ground, that is, on the border of two black soil strata lying one on top of the other, as they do over the whole of Podolia. I would point out that pottery and finds from later times are found exclusively in the upper black soil stratum, and do not penetrate to the lower.

Another fact which assisted me in the proper dating of the black graphite pottery was the discovery among the pottery from one of the settlements examined by me in Nowosiółka Kostiukowa, of two bronze fibulae of the Peschiera type. Some years later there was found in this same neighbourhood a bronze treasure dating from the Third Montelius Period. These bronze articles had been placed in a clay vessel exactly similar to the graphite pottery known from the settlements in Nowosiółka and Holihrady. In the light of these facts, there can be no doubt that the black graphite pottery in Podolia is considerably older than I at first supposed. It quite certainly dates from about 1200-1100 B.C.

If we also take into consideration the statement of Hubert Schmidt, that painted pottery in Cucuteni remained in use until 1200 B.C.[2] and also the fact that

[1] As yet unpublished. I mentioned it and gave the cross-section of strata in my article "Climate and Population" (*Baltic Countries*, Vol. I. Toruń-Poland, 1935).

[2] H. Schmidt : *Cucuteni in der Oberen Moldau, Rumänien.* Berlin-Leipzig, 1932.

both the technique of the articles and the form of the graphite pottery vessels are similar in every respect to the painted pottery, we must come to the conclusion that in fact there is no gap of time between these two types of pottery. One is simply a continuation of the other.

I should add that the black graphite pottery appears all over this area, as does the painted pottery (fig. 1). It is known from a number of settlements in Moldavia, where, for instance, in Cucuteni, it also appears in settlements where painted pottery is found. In the Ukraine, graphite pottery is considered as pottery of the Hallstatt period, on the grounds that it is known from Scythian graves, as well as from settlements.

I do not intend, however, to engage here on a longer discussion of this pottery. I should only like to state that this is an important problem. For its solution, and for the definition of the extent, genesis and chronology of the graphite pottery, co-operation of the archaeologists of all three of the regions concerned, that is, Rumanian, Polish and Ukrainian, is indispensable.

I pass on to another group of problems : problems extending beyond the black soil area and embracing the Hungarian Plain.

Some years before the present war, a number of articles was published dealing with a very interesting group of finds from Pre-Scythian times, but nevertheless possessing a mixed character, with a preponderance of eastern elements.[1] To this group belong, among others, such famous gold finds as those of Mihałków, Fokór and Dalj. Besides these, there are a whole number of bronze finds and some barrow-graves. Their geographical extent is considerable, embracing south-eastern Poland, Rumania, Transylvania and Hungary. Finds of this kind also occur as far afield as Austria, Czechoslovakia and Slavonia. Following the suggestion of P. Reinecke, who was the first in his time to turn his attention to these finds, they have been classified under the name of " Thraco-Cimmerian " finds.

On the basis of the material so far available, it may be suggested that we have to do here with the culture of a higher caste, and not with that of the common people. It seems to me that the study and clarification of the character of this group is very important. It should facilitate the elucidation of the end of the Hungarian Bronze Age, and the problem of the beginning of the movements of the Scythians. It is a well-confirmed fact that in Central Europe there are no finds of certain Scythian origin earlier than the beginning of the sixth century B.C. On the other hand, it is also absolutely certain that, in the areas east of the Danube, there must have occurred some great ethnical movement and political changes before this date. This results from the fact that in those areas, including the Ukraine, there is no

[1] D. Berciu : " Ein hallstättisches Brandgrab aus Balta-Verde." *Eurasia Sept. Antiqua*, vol. 9 (1934) ; J. Nestor : " Ein thrakokimmerischer Goldfund aus Rumänien." *Eurasia Sept. Antiqua*, vol. 9 (1934) ; The same : " Zu den Pferdegeschirrbronzen aus Stillfried." *Wiener Präh. Zft.*, vol. 21 (1934) ; The same : " Der Stand der Vorgeschichtsforschung in Rumänien." *Berich d. röm.-germ. Kommission*, vol. 22 (1933) ; T. Sulimirski : " Die thrakokimmerische Periode in Südostpolen." *Wiener Präh. Zft.*, vol. 25 (1938) ; K. Willvonseder : " Ein Depotfund aus Stillfried." *Wiener Präh. Zft.*, vol. 19 (1932).

trace of western imported articles after 800 to 700 B.C.[1] In this period the contact, until then close, between these countries and the west must suddenly have been broken off. But just at this time, between the breaking-off of the contact with the west, and the oldest Scythian finds, are dated the Thraco-Cimmerian finds.

If, in our review of important and urgent problems, we advance further into central Europe, the key problem in this area will be that of the Lusatian culture. This culture is divided into a number of smaller local groups, of which some have already been conclusively worked out. Nevertheless, the Lusatian culture has never been worked out as a whole, although an enormous quantity of material exists, of which a considerable part has been published. A number of conflicting opinions exist as to which of these local groups should be included in the Lusatian culture. Hence there also arise great discrepancies in determining the extent of this culture.

I consider that the working out of the Lusatian culture in its entirety is one of the most urgent tasks confronting central European archaeology after the war. Since this culture embraces several countries in its reach, wide co-operation will be necessary. Before the war, it was completely impossible on account of conditions in Germany, and the political stamp which was given by German science to the problems of the Lusatian culture. The working out of this culture, the establishment of an incontestable chronology for it and the elucidation of its downfall will contribute in considerable measure to the fixing of its ethnical origin.

Last, I should like to touch on a whole group of most interesting problems which have arisen relatively recently, which in my opinion, have wide importance, and which, moreover, cannot be solved within the limits of one nation.

In the area of south-eastern Poland, there existed at the close of the Bronze Age and the beginning of the Iron Age a small local group, known as the Wysocko culture (fig. 1). At one time it was considered as a peripheral group of the Lusatian culture.[2] However, my investigations, published in 1931[3] showed that this culture is completely separate from the Lusatian culture, and the Lusatian elements it contains, chiefly bronze articles, are simply the result of the powerful influence of a neighbouring culture of higher level.

Just after the appearance of my publication, some new graves were discovered, and settlements of a Wysocko culture in the Polish part of Volhynia. Later, in 1934, during my journey in the Soviet Union, I found in the Museum in Żytomir inventories of the graves of that neighbourhood. They were absolutely analogous, in character to the types known from the Wysocko culture, but did not, however, possess Lusatian elements. As a result of study of other exhibits from some North Ukrainian museums, in particular in Żytomir, Berdyczów and Czernihów, I have

[1] J. Nestor: Der Stand der Vorgeschichtsforschung in Rumänien. *22. Bericht der röm'sch-germanischen Kommission.* Frankfurt a/M. 1933. p. 106. A. Szlankówna: " Kilka importów staroitalskich i zachodnio-europejskich z płd.-wsch. Polski i Ukrainy" (Some old-Italic and western-European Imports from south-eastern Poland and the Ukraine). *Swiatowit,* vol. 17. Warsaw (1938).

[2] Kostrzewski, J. : " Ostpolnische gemischte Gräberfelder." *Reallexikon d. Vorgeschichte* (Ebert), Berlin, 1927, vol. 9, pp. 244-246.

[3] T. Sulimirski : *Kultura Wysocka* (Wysocko Culture), Cracow, 1931.

come to the conclusion that we have to do here with some major cultural entity.

During my short stay in the Soviet Union, I was unfortunately not able to investigate thoroughly either the eastern or the northern limits of this cultural entity. In the south it extends only to the border of the black soil region, on which the painted pottery culture had already been developed, and where is also found the subsequent black graphite pottery culture which I discussed earlier in this lecture. Its most western group is that of the Wysocko culture (fig. 1).

These cultures, related to the Wysocko culture, I called in my article published in 1938[1] " Volhynian cultures ".

In connection with these Volhynian cultures, two important problems emerge : the first is the problem of chronology, the second ethnical.

The Volhynian cultures, like the Wysocko culture, have a distinctly Neolithic aspect. Within the limits of the Wysocko culture, both in the graves and especially in the settlements, great quantities of flint tools are found. In the settlement which I came upon in the neighbourhood of Złoczów only small iron knives and needles testified that we have to do not with a Neolithic settlement but with one of the Early Iron Age. But neither the flint articles nor the pottery from the settlements of the Wysocko culture differ in any respect from the pottery from the settlements of the so-called " Bug Stone Age " culture, which Professor Kozłowski[2] placed in the Third Neolithic Period according to Montelius.

It is characteristic that from the settlements of the Wysocko culture so far discovered, there are only very late types of bronze articles, actually belonging to the Early Iron Age and some iron articles. It is possible to conclude from this that there was no Bronze Age at all in that part of Poland, but that the Iron Age followed immediately on the Stone Age.

From the economic point of view, such a development is quite comprehensible. On the territory occupied by the Volhynian cultures, very large quantities of excellent flint are found. Articles from this district made from flint and finished with excellent technical skill are found as imports even in western Poland. On the other hand, there is absolutely no copper or tin found in these regions. The bronze articles must therefore have been brought from a distance, and for that reason must have been too costly to come into general use. It is quite otherwise with iron, for this country is rich in surface ores, from which iron was smelted up to relatively recent times. Furthermore, one must reckon with the great conservatism of this people, which even to-day is depicted in all their material culture.

If, however, such persistence of the Neolithic culture is assumed, a new problem again arises, that is, how to establish an absolute chronology for such conservative and backward cultural groups.

[1] T. Sulimirski : " Problem ekspansji kultury łużyckiej na Ukraine " (The Problem of the Expansion of the Lusatian Culture in the Ukraine). *Wiadomości Archeologiczne* (*Archaeological News*), vol. 14, Warsaw (1936).

[2] L. Kozłowski : *Młodsza epoka kamienna w Polsce* (The Late Stone Age in Poland). Lwów (1924).

I believe that the stratigraphy of remains found in various types of soil can give a certain amount of assistance in these difficulties. A similar method has been applied to finds in peat. It may be possible to work out a similar method for the black soil strata, and for other soils, and also for sand dunes. It is obvious that in the working out of a chronology based on soil stratigraphy, the co-operation of the scholars of all eastern European countries would be indispensable, and so also that of archaeologists and specialists in other subjects, such as soil experts, botanists, etc.

The second problem which arises in connection with the Volhynian cultures is that of their ethnical origin. In this connection, I submit the following observations.

It is a fact that the pottery of the Wysocko culture is identical with that of the Bug Stone Age culture. This last has a close connection with Russian Neolithic pottery, that is, with Comb ware pottery and other types deriving therefrom. It appears from this that the Volhynian cultures belong rather to the north-eastern cultural groups of Europe.

Yet another fact supports such a supposition. Professor Czekanowski, Professor of Anthropology in the University of Lwów, published in 1935[1] a map of the extent of various ethnographical phenomena in the contemporary material culture of Poland. It is very significant how all the lines for the main part run parallel forming an unusually interesting border line, which runs diagonally across the whole of Poland from Dantzig to Lwów, and thence straight eastward (fig. 2). This border line does not correspond to present day national frontiers. In the north, it cuts through territory ethnically densely Polish, and in the south, Ukrainian. Nevertheless, this line is in accordance with the extent of prehistoric cultures. In particular, it covers in the west and south the boundaries of the Volhynian cultures.

On the basis of the extraordinary persistence of this line, which does not accord with present conditions, but accords with the prehistoric position, Professor Czekanowski comes to the conclusion that the line must be not only very old, but also a very real frontier. It most clearly separates the north-eastern forest area from the rest of Europe in the west, and from the steppes in the south. This very forest area of northern Europe formed the ancient territory of peoples of Finno-Ugrian stock. South and west of this line Indo-European peoples undoubtedly lived. In the west, independently of whether or not we are to consider the Lusatian culture as ethnically Slav, there is in any case no question that this was the culture of some Indo-European people. In the south the black soil regions were occupied by Iranians, likewise an Indo-European people.

The Volhynian cultures should therefore be considered as cultures rather of Finno-Ugrian stock.

[1] J. Czekanowski : " Zróżnicowanie etnograficzne Polski w swietle przeszłosci " (Ethnographical Differentiation of Poland in the Light of the Past) in Polish only in : *Reports from the Sessions of the Polish Academy*, vol. 40, Cracow, March (1935).

It is clear that in this field also only further investigation and the co-operation of the scholars of the countries concerned will lead to the definite solution of ever new problems.

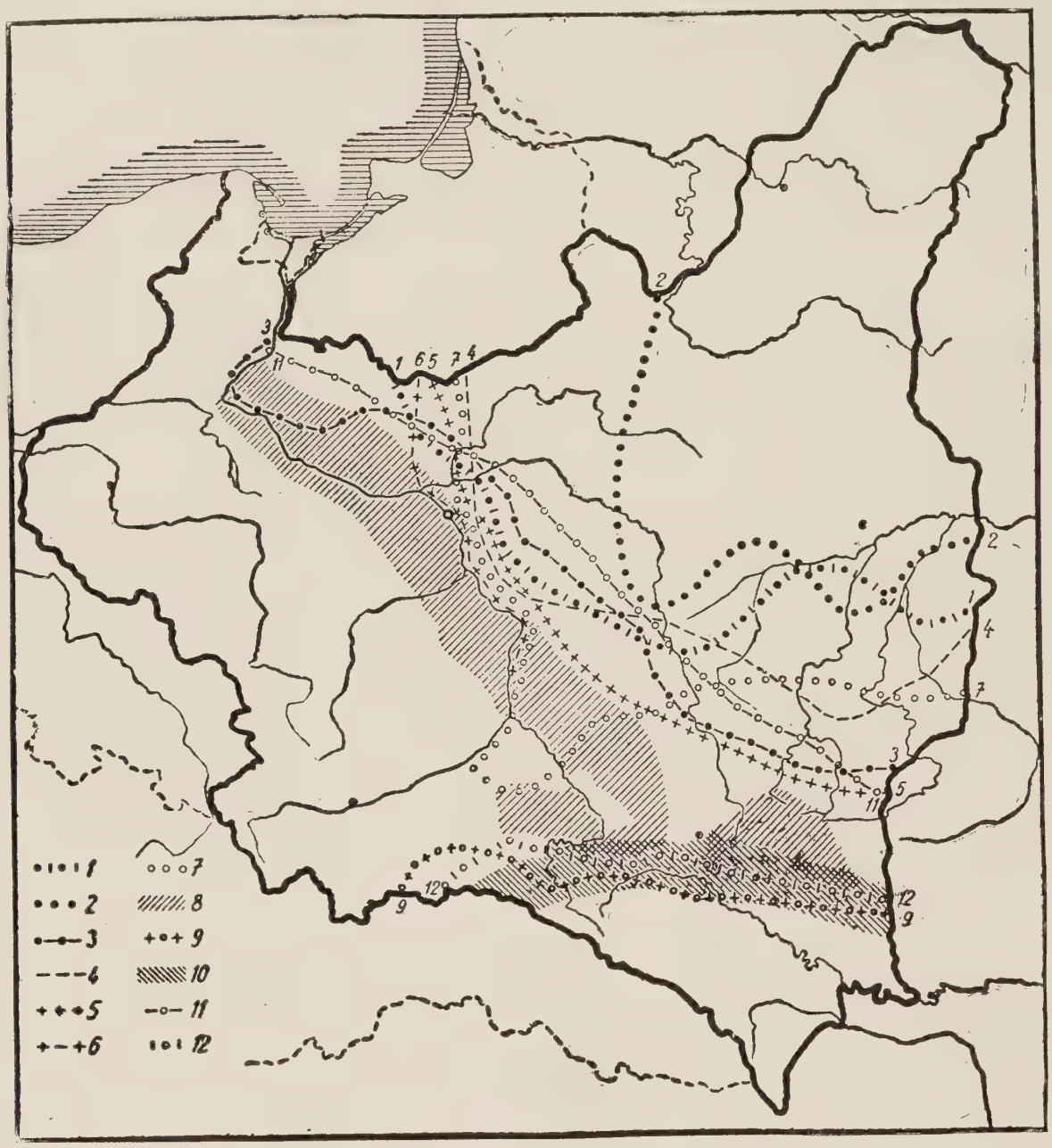


Fig. 2. Boundaries of some ethnographic phenomena in Poland (after Czekanowski).

1.—Boundaries of two kinds of flail.
2.—Boundaries of two kinds of distaff.
3.—Boundaries of two kinds of tadpole.
4.—Boundaries of two kinds of yoke.
5.—South-western limit of " duha ", a type of harness.
6.—Boundaries of two kinds of roof ornaments.
7.—Southern and western limit of " socha "-type of plough.
8.—Eastern limit of the Lusatian culture (it runs in fact a little further east, as shown on fig. 1).
9.—Northern limit of southern type of distaff.
10.—Northern limit of " Hungarian " type of finds from Bronze Age.
11.—South-western limit of a type of stamp-trough.
12.—Northern limit of a type of stamper.

## Discussion

*Professor V. Gordon Childe.* I was particularly interested in the portion of the paper dealing with painted pottery. Years ago I thought that it was Neolithic, but it now looks as if it is quite possible that there is no yawning chasm of time separating it from the Hallstatt period. Further, it is now not impossible that the Tripolye culture lasts until quite late in the Bronze Age.

*Mr. Miles Burkitt.* At Cambridge we have a big collection of painted pottery from Niezwiska. Difficulties will arise if all the painted pottery is to be dated to the Hallstatt period, as field notes we have show Hallstatt material overlying the painted ware. I presume that the suggested Early Iron Age dating refers only to the B and not to the A ware ?

*Mr. D. A. J. Buxton.* What a pleasure it is to me to have Professor Sulimirski direct our interest to the painted pottery of eastern Europe, because I am convinced it has suffered from an unnecessary neglect from archaeologists. I cannot say why, unless it is because this is a country far away and there are language difficulties, and I think people tended to turn away from it because of the difficulty in the very names themselves, but even these are only there to be overcome.

I should like to point out that it seems to me it affects European archaeology as a whole. There is the possible connection with the early Thessalian pottery. If you are to have a chronological scheme it must fit in with other European cultures.

*Dr. S. Weinberg.* The last speaker's reference to the painted pottery of Thessaly is the second reference to Greece ; Mrs. Hawkes mentioned the neolithic remains from Crete. Therefore, I think that at this point it might be well to give a brief statement of the position in the Aegean.

The Aegean is beginning to show numerous specific connections with the Near East, many of which are the result of the recent excavations at Troy, Tarsus and Hissar in particular. Such connections are helping us to establish more exactly the prehistoric chronology of the Aegean area. There are also numerous points of contact with S.E. Europe, and the closer establishment of Aegean chronology will in turn help to define the chronology of S.E. Europe.

It is at the period ca. 2300 B.C. that the best chain of evidence connects the Greek sequence with that of the Near East. Just at this time there appears in Thessaly an abundance of painted pottery of S.E. European type. The chain of connections is not complete, but there is strong evidence that the missing link is to be found in Thrace and Bulgaria. The same period saw the appearance of strongly Danubian pottery types in Macedonia. It is, therefore, in the region of Macedonia, Thrace and Bulgaria that we need large scale excavations rather than pit digging, for by such excavations only shall we be able to solve many of our own problems and to contribute something to the solution of some of the problems in S.E. European prehistory.

*Reply by Professor Sulimirski.* The settlements A and B at Niezwiska on the river Dniester were situated in quite different places and have no connection. During my research in south-eastern Poland I came to the conclusion that there was no break between the painted pottery culture (type B) and the culture represented by the graphite pottery, wrongly considered as a Hallstatt period pottery. No change of population took place at that time, but simply a change of pottery decoration while the shapes of it remained to a great extent the same.

# PROBLEMS OF THE BRONZE AGE AND THE BEGINNING OF THE EARLY IRON AGE IN WESTERN EUROPE

*Chairman :* The Rev. M. P. Charlesworth, M.A., F.B.A., F.S.A.

*Speaker :* C. F. C. Hawkes, Esq., M.A., F.S.A.,
*Assistant Keeper, Department of British and Medieval Antiquities, British Museum*

From the zenith of the European Bronze Age, in the time of the Mycenean Civilization, to the zenith of the Iron Age of historical antiquity in the establishment of the Roman Empire, are twelve hundred years, and of the archaeological problems of western Europe in these twelve centuries, I can obviously speak this afternoon of no more than a few.

The most obvious characteristic of this period as a whole, from the west European point of view, is a familiar one, especially to British prehistorians. It is the march of events towards the west from centres lying to the eastward. And those centres are broadly speaking, three : the eastern Mediterranean, the home of the first higher or literate civilizations on European soil ; central Europe ; and lastly, the Germanic and Scandinavian North.

Of the first, Dr. Schaeffer has already had much to say for the Bronze Age ; as has Dr. Clark of the earlier prehistory of the last. I shall then begin where my field borders upon Professor Sulimirski's, that is, in central Europe.

Now in 1200 B.C. the peoples and cultures of central Europe were already deeply stratified. The High Bronze Age itself displays two great cultural systems, the Tumulus Cultures and the Urnfield Cultures, distributed as shown on Professor Childe's familiar map. But beneath the surface, so to speak, of each, lies a varied pattern of substrata left by the Neolithic and the turbid age of transition to the Bronze Age from it. Broadly speaking, the Urnfield cultures are the product of a blending between intruders of that transitional age and the ancient Danubian civilization. In the Tumulus cultures the intruders stand out more sharply, their dominance typified, here as in Britain, by their Tumulus or Round Barrow burials ; but beneath them are also substrata from the Neolithic, of mainly non-Danubian cultures such as the Michelsberg and Altheim.

Therefore, when we come to study the movements of our period out of central Europe towards the west, we have to realize that these were not movements of compact and homogeneous blocks, like the Children of Israel in the book of Exodus. The primary movements themselves will have been movements of mixed and stratified communities ; and, more than that, it will always be a question whether, at any given point, a primary movement may not already have handed over to a secondary one, of peoples whom it has set moving in their turn, carrying some of the superficial characters of its culture, but recognizable as secondary by their

continued retention of some of their own. And, of course, there is always also the further question, whether at any given point there is any movement at all, and not rather merely a process of cultural, industrial, or commercial diffusion.

My first proposition, then, is that the conventional units in the period's prehistory, if we are to know where we are with them, must first of all be analysed by reference back to earlier periods, in order to disclose their composition. Professor Sulimirski has already given a leading instance of this requirement : that of the Lusatian or Lausitz culture. From the east-European point of view, there is an arguable case for seeing in the Lusatian peoples between the Oder and the Bug the ancestors of the Slavs. The official German doctrine, on the other hand, is that these and all true Urnfield peoples were Illyrians. But who is to say what degree of cultural uniformity within the Urnfield cultural system corresponds to ethnic and linguistic unity ? Are the Knóviz people in Bohemia the same people as the Lusatians of the Lausitz itself ? And what about the peoples of the north Alpine, Rhineland, and more westerly Urnfields ? And those of Hungary ? and Italy ? Professor Pokorny, formerly of Berlin, produced a book shortly before the war in which linguistic was added to archaeological material for the demonstration that the whole Urnfield movement, from the Lausitz outwards, in all its directions, was a great expansion of Illyrians. But in western Europe at any rate, the Urnfield immigrants are usually taken to be Celts, and the Bronze Age Tumulus peoples Proto-Celts, who after receiving into their bosom primary movements of Urnfielders, became fully-fledged Celts and so migrated westward in their turn. What is the right answer ? It must affect one's whole view of the ethnology of western and west-central Europe.

I therefore very warmly second, from the western standpoint, Professor Sulimirski's plea for international collaboration on the whole Lausitz and Urnfield question. And I should like to illustrate the importance that I have claimed for an analytical approach to it by a map (fig. 1), which was prepared for the Oslo Congress in 1936. The leading pottery type of the British version of the Urnfield culture, the Deverel-Rimbury urns of our Late Bronze Age, are not proper to the original Urnfield peoples at all. As the series of drawings below the map shows in chronological order, they are the direct outcome of the local Neolithic tradition of N.W. Europe between the Rhine, Normandy, and Holland, and are thus the contribution neither of the original Urnfielders, nor of the Tumulus peoples on whom those first impinged, but of the local substratum-population of Neolithic descent, who there found themselves caught up in the later stages of the westward Urnfield movement. Yet these urns are most often the only evidence that we in Britain can show for the arrival of that movement on our shores.

Edified by this lesson, then, we can make a second proposition : that if we are to come to terms with the question of immigrations in this period into western Europe, we must not only analyse the immigrants, but pay at least equal attention to the " immigrees ", that is, to the older western populations, and their reactions to the immigration process.

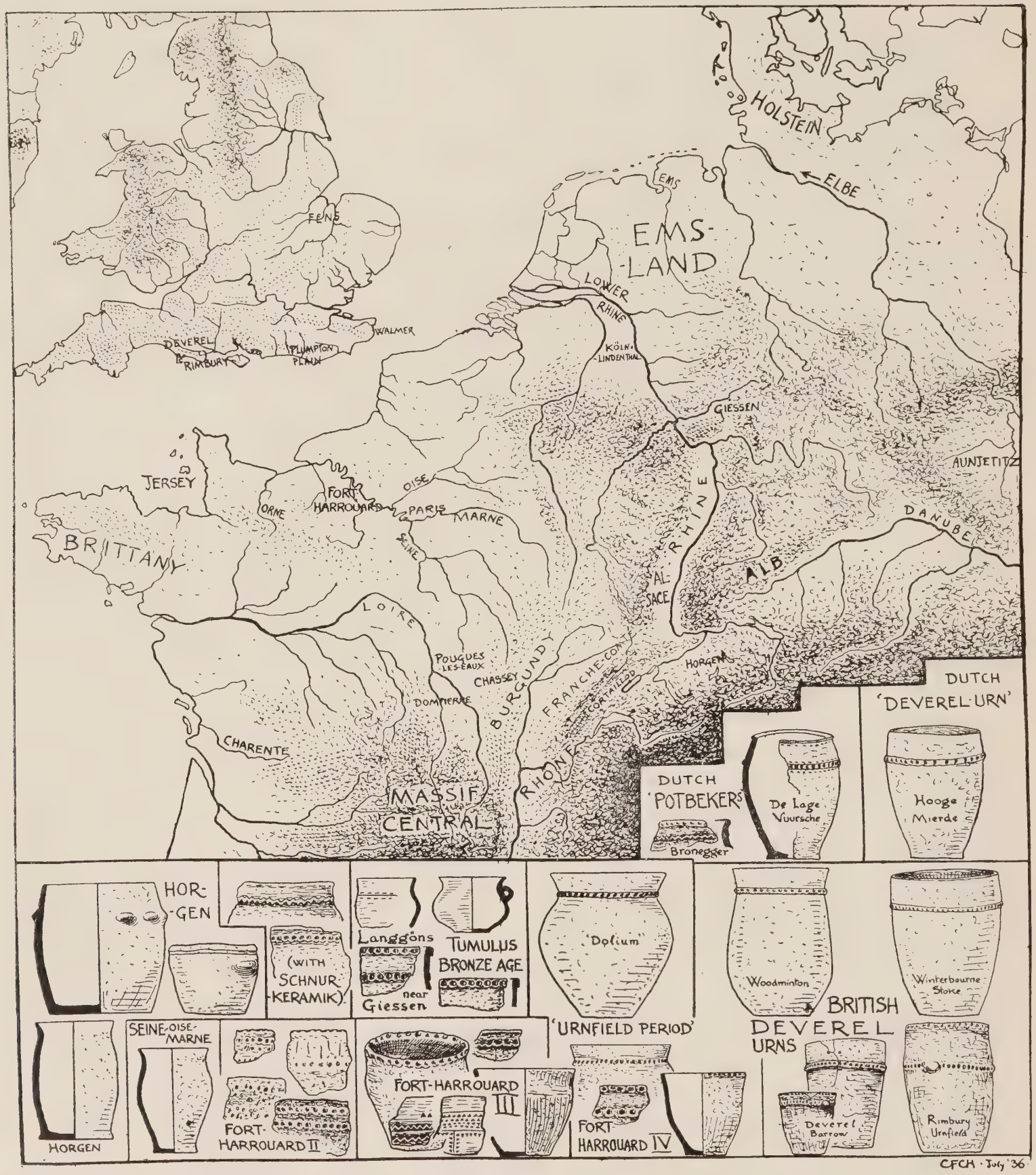


FIG. 1. Map of N.W. Europe, showing some places of importance in the Bronze Age—Iron Age transition, with series of drawings showing the Neolithic and Bronze Age ancestry of the Dutch and British 'Deverel Urns'.

And if this is important when working with pottery, it is still more so when working with bronzes.

Central Europe had its own industrial and commercial system for bronze, based on its own central-European deposits of copper and tin. But so also had the West, and in particular the North West, based largely on the copper of Ireland and the tin of Cornwall. Late Bronze weapons like ours from Britain thus display a blending between foreign traditions and native, and the effective study of the types demands something more than just brute typology. At the same time it remains of course true that if properly handled, the study of bronze types has yet a great part to play in the anatomizing of the period. And it becomes particularly important when, first in south-eastern and then in central Europe, bronze began to give way before the competition of iron.

It is a shocking truth that there is no comprehensive modern work on the propagation of the iron industry in prehistoric Europe. But the man who in his prime could have written it best is Reinecke; and although he has never done so he has probably done more than any other man to shape the modern conception of the first transalpine-European Iron Age—the Hallstatt period. The leading fact, which was well expounded also by Déchelette, is of course that after its appearance before 1000 B.C. in the Aegean world, the knowledge of iron proceeded in Europe by way of Italy and the Adriatic. It was thus that the iron-ores of the eastern Alps, near which Hallstatt itself lies, came first to be exploited. But the establishment of the industry was in general preceded by the scattered introduction of individual iron objects; and since in central Europe these appear first in the mature phase of the Urnfield culture, Reinecke named that as the first stage of his chronological system for the Hallstatt period. This "Hallstatt A" runs alongside the last stage " E " of his system for the Bronze Age. Thus an essentially Late Bronze Age type like the antenna-hilted sword becomes equally a " Hallstatt A" type of the Early Iron Age. Reinecke thus ingeniously made the two chronological systems oversail one another, in order to represent the overlapping of the latest Bronze Age Tumuli by the Urnfields, which then lead on into the Iron Age. It was a valuable landmark in the progress of archaeological thought towards classification by cultures rather than by mere absolute dating periods. But it has since become a frightful nuisance. For above all, see what happens when we get to the next stage, " Hallstatt B ". Amongst the material of Reinecke's type-series for this "Gündlingen" stage, the typical long Hallstatt sword now appears, both in bronze and iron. But in iron, this is really the characteristic weapon of the next stage again, " Hallstatt C ". And on the one hand, Kraft has shown that in its classic region, of Württemberg, this " Hallstatt C " is the earliest Hallstatt culture stage that there is at all; while on the other, its advent does nothing to stop the swords, in many other regions, from continuing to be made in bronze. The explanation of this seeming muddle is, of course, the continued co-existence of the Tumulus and Urnfield culture-traditions side by side: the Gündlingen or B pottery is really Urnfield pottery somewhat modified by Tumulus influence, whereas the

C pottery comes from Tumuli, and shows the old Tumulus tradition somewhat modified by Urnfield influence. B and C can in fact be at least as much contemporary as successive. And if this makes the Reinecke system unreal and awkward for central Europe, it makes the study of Hallstatt diffusions in western Europe on these lines something like a terminological nightmare. It is rumoured that during the war an authoritative new book on the Hallstatt period in Germany has been published there, by that upright veteran of the old German learning, Professor Merhart von Bernegg. I much hope that this may have set the tangle to rights. But in any case, the period has to be understood outside Germany as well as inside, and here accordingly is my third proposition.

The whole Hallstatt business wants a thorough overhaul, in which three quite different things must be kept distinct ; first, the industrial and economic history of the supervening of iron upon bronze ; second, the cultural and social history of the Tumulus, Urnfield, and other communities of *people*, both in central Europe and the West ; and lastly, as a common measure to be applied to each, the absolute chronology of all these centuries, from 1200 to 400 B.C.

To the communities and peoples I shall shortly come back, but I must at this point remind you, that pursuit of them cannot be confined only to one side of the Alps. Of the iron industry, I have already pointed out that it came to inner Europe from the direction of Italy ; and as for the absolute chronology, the plain truth is that nearly the whole of its immediate basis is and must be Italian, and that the Italian archaeology of this period has latterly fallen, amongst all of us but a very few, into a very lamentable neglect.

The old men were not here to blame ; Montelius and Sophus Müller, Déchelette, and Reinecke conspicuously, all recognized in Italy the corner-stone of their chronological systems. But what has happened since their day is this. The chronology of Iron Age Italy depends chiefly upon that of the imports into Italy, and of the imitations and renderings made in Italy, of datable objects from Greece and from the Orient. And while most transalpine archaeologists have gone on dating these as they were dated forty and fifty or more years ago, Greek and Oriental archaeological studies have advanced, and have left many of the old attributions and datings behind. A few have realized this. But the most notable of them, Dr. Åberg, has taken the bit between his teeth and pulled his Italian and his Hallstatt datings down far more drastically than I believe to be warranted. And nobody, unless Professor Merhart has now done so unbeknownst, has really drawn for the benefit of European prehistory at large upon the best modern work on either the Italian material itself, or the Greek and Oriental material behind it. I am thinking in particular of that of Payne and of Blakeway on Archaic Greek and especially Proto-Corinthian and Corinthian pottery. To these we must now add Miss Dohan's recent work on the Italic tomb-groups at Philadelphia ; but obviously I cannot go into it all here. What it comes to is this—there are two matters to be studied : the proper dating, on the evidence mainly of the Greek material, of the classic Italian culture-stages ; and the proper

inferences to be drawn therefrom for the dating of things in inland Europe by association with imports out of Italy across the Alps. And on that second matter one word in conclusion : a culture need not begin to exist only when it begins to import datable objects from its neighbours. True, the main flood of imports across the Alps no doubt begins with the latest Benacci and early Arnoaldi periods at Bologna, towards 700 B.C. But something must then have been already in existence in order to attract them ; and of course some earlier imports across the Alps are known—indeed, as Dr. Forssander reminded us in 1939, they go back to the pre-Benacci Bronze Age, with its early fibulae and its type-sites, such as the Pianello di Genga, for the Italian branches of the Urnfield culture. To understand the Hallstatt world at its apogee from 700 onwards, one must consider its earlier development, in the centuries over which Iron Age and Bronze Age studies have to join hands. And, of course, our concerns in the remoter north and west, with their often so much less attractive material, depend on these studies for their background. Through our familiar ugly pots, the dating of a sword or pin type may give a historical setting for much that we can study nearer home, in the out-of-door archaeology of excavation and other field-work in which north-western Europe in this last generation has begun to make such progress. Our progress with natural stratigraphy and pollen-analysis, too, comes into the picture ; for instance the famous horizon of climate-change in the Scandinavian bogs, the *Grenzhorizont* which we have begun learning to recognize in Britain and in Ireland, depends for the finer limits of its primary dating in Denmark upon bog-finds of objects that must look, for their chronological value, to Italy.

I do not want further to pursue this indoor or museum side of my subject. You all know that the Italian and Mediterranean influences upon Hallstatt Europe led presently to the great Celtic art of La Tène times, the centuries that bring one at last to the age of the Roman Empire. And doubtless you all know also, that on the subject of that art one of the monumental books of our time, by Dr. Jacobsthal, is on the very point of publication. But the world of the Celts and the Germans whom the Romans came to conquer or confront was to a great extent made not in La Tène times, or by the La Tène culture, but in the preceding centuries of which I have been speaking. Mr. Ward-Perkins showed well, in his recent paper on the Gergovia pottery, that the cultural bedrock of Celtic and Roman Gaul was in the main not La Tène but Hallstatt. Our so-called Iron Age A in Britain, of which the same is so true, is essentially a Hallstatt derivative. And the work we had begun to do on it here just before the war, in the partnership between the Prehistoric Society and Dr. Bersu at Little Woodbury, not only showed the mature vitality in it of an agricultural tradition that goes back to Hallstatt and Urnfield times on the Continent and ultimately to the Neolithic Danubians, but also what excavation can do to bring to light the social life and institutions of prehistoric people. Progress on such lines has still barely begun : Little Woodbury itself is not yet fully excavated, and it is a single-homestead site, which closely adjoins the much greater hill-fort of Woodbury proper. There there should be

more work still, and by such means the hill-forts, the sampling of which by excavation has been for so many of us in this country an apprenticeship to archaeology, will one day be enabled to take their place in a fuller human understanding of the societies that made them.

My fourth proposition then, is this : since in the life of prehistoric western and west-central Europe it is the full pre-Roman Iron Age that should display the greatest maturity of tradition, that Age is in every region worth the fullest study, in the light both of all the knowledge available of the preceding Ages, and of all the comparisons available between one region and another. Dr. Schaeffer will know that in the last regard I am thinking most of all of Britain and of France ; but everywhere the countries of Europe are each other's neighbours, and the thought can surely apply to all, as of course it does between different regions of the same country. How good it would be, for example, if to Dr. Schaeffer's work on Bronze and Iron Age Alsace and Mlle. Henry's on the Côte d'Or there could be added, to begin with, a third on Franche Comté and the Jura, where M. Piroutet and others have found so much, but of which the world has still so little systematic knowledge !

Now I have one more proposition left, and I will ask you to hear me out on it for the special reason that it brings me to the one quarter of western Europe I have not yet mentioned : the Iberian Peninsula. I do not know if we all realize the extent of this country's good fortune in being able to publish the two major studies of Iron Age Spain completed by Professor Bosch-Gimpera during his recent sojourn with us. That on the Iberians and their painted pottery, which will be appearing shortly in *Archaeologia*, I shall leave to speak for itself. But that on Celtic Spain, which the British Academy have already published, brings me to end on a matter that I hope Mr. Charlesworth will feel to be as important as I do.

You all know the names of the principal tribes of Iron Age Britain as the Romans knew them. Many of the names occur also on the Continent, and archaeology ought to be able to identify the movements which carried them here from there. For to ask " Where were the Brigantes ? " is to receive an answer not only from Huddersfield, but (and more recognizably) from Bregenz on the Lake of Constance. Bosch-Gimpera plots all the recoverable tribe-names of Celtic Spain, and seeks to identify the migrations that brought them from here in Belgic Gaul, or from still further away in Europe, in and before the sixth—or at the latest the fifth—century B.C. Maybe we in Britain may later on feel able to try the same game, as we have very easily been able to do for the Belgae who came here from Gaul in the first century B.C. But it is not simply the migrations themselves that make this worth doing : identifying them is a means to a further end. If the date of a migration, say, of the *Ambiani* from the neighbourhood of *Amiens* to that of *Ambiona* in the Spanish province of Burgos can be dated archaeologically to a round date as early as, say, 500 B.C., and confirmed by similar demonstrations for other tribes, then we have surely a proof that tribal entities authenticated in Roman times go back a very long way, through the La Tène into at least the Hallstatt

period. And thereby not only are ethnology and philological science stimulated and enriched ; but archaeology itself is enlightened in no matter what country of Europe, by seeing that the solidarity of ethnic groups, as Professor Sulimirski would claim for his Painted Pottery people, may transcend those changes in the outward apparatus of material culture, to which archaeologists perforce pay so much attention. With that solidarity, too, will have gone some degree of continuity in institutions, which in their material embodiments the archaeologist should then use his appropriate techniques, if possible, to help detect. Probably he will learn a great deal in the process. And certainly he will be able to give a far better account of himself than I fear he can at present, when the Roman historian comes over to remind him that the peoples of his cultures and his movements are the raw material of an Empire, and what has he got to say about them? Moreover, behind the Roman will come the Medieval historian, with a very similar question on his tongue.

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The present author's paper on " The Western Bronze Age and the Celts ", read at the International Prehistoric Congress at Oslo in 1936 accompanied by the map now published here (above, p. 52), was in the main embodied in his article, " The Deverel Urn and the Picardy Pin " in *Proceedings of the Prehistoric Society* viii (1942), pp. 26-47, esp. pp. 39-44.

## DISCUSSION

*Dr. C. F. A. Schaeffer.* Mr. Hawkes has reminded us of the quality of European bronze work. One of the illustrations showed a bronze girdle, which developed in central Europe. From there it was brought as a fashion to the Near East and is found as far afield as Persia. We must not forget, therefore, that Europe has made contributions to the Near East as well as vice versa.

*Professor Sulimirski.* I would like to emphasize the unity of Europe as a whole during the Early Iron Age. Imports from Italy, dating from this period, were found as far

afield as the Ukraine. Definitely Italic articles have been found in Podolia ; also imports from Brittany have been found in Poland. This is an important basis for the establishment of proper chronology and confirmation as to which cultures in the east of Europe were really contemporary with those in southern and western Europe. There is, however, one serious difficulty, which is that the types of implements found in graves or settlements often do not correspond to those found in the treasure-hoards. But it was just the latter which for the most part contained the foreign imported wares. The collaboration of archaeologists of different countries would greatly help the establishment of a satisfactory chronology. Archaeologists working in western Europe should have more knowledge of eastern Europe. Those working in the east have a much better knowledge of what is being done in the south and west.

*Dr. F. M. Heichelheim.* In connection with the Celtic tribes and, on occasion, much smaller units which reached this country, a comparison may be made with the settlements of the so-called American Frontier. Identity of name does not prove the place of origin, or only for a small percentage of the new population. There may be some other reason for the similarity. To establish a real connection, additional evidence must support etymology. Excavation must show connection in objects or cults which are not too widespread in their occurrence.

*Mrs. Chitty.* Mr. Hawkes' inspiring paper not only synthesizes important problems on the Continent, but lights up the most parochial corners of our studies. We all feel that what he has to say is applicable to something in our own experience, and makes us look at that experience with new eyes.

He speaks of the complex of pre-existing cultures caught in a great advancing movement to such an extent that, for instance, by the time the Urnfield culture reaches Britain, the pots characteristically found in the urnfields are not of original Urnfield type at all. In remote corners of the Pennines this process has gone further, and the burial urns found disposed in urnfields are of pure British types.

He speaks of the Hallstatt background of Roman culture. This is so marked in E. Yorkshire that a tradition of potting that we can only call " Hallstatt " at present survived all through the Roman period to the Saxon settlement.

When that tradition of potting arrived and what it meant, why at this point in E. Yorkshire (the beginning of " Hallstatt " culture) the native Bronze Age amalgam seems to disappear and its successor seems to be so long-lived, are questions affecting our estimate of the civilization found by the Romans in the district and of what was the Roman achievement here.

*Mr. Miles Burkitt.* Is it not possible that increasing dampness at the end of the Bronze Age had something to do with changes in the cultural conditions ? Increasing forests may have restricted living space and brought communities closer together. This would have resulted in the merging of local knowledge, and in the development of nationalities, in contrast to the individual settlements existing under the easier conditions of the earlier part of the Sub-Boreal phase.

*Dr. Grahame Clark.* I think the lecture to which we have just listened has been a revelation of the difficulties which we have to face in tackling the problems which Mr. Hawkes has so well indicated and illuminated. As we come to grips with the problems defined by the Chairman of associating archaeological material with the peoples who emerge in the first light of history in this part of the world, we are brought face to face with fundamental problems of interpretation. For example, what phenomena are we to interpret in the light of movements of population, what as due to changes of fashion and what as reflecting modes of economic life subject to their own development ? All this underlines the necessity of getting to grips with the bed-rock reality of social structure.

The interpretation of the meaning of types of objects found is going to be very different

in the case of a peasant community, isolated by difficult communications, from what it is in another community more stratified and more specialized in function.

The study of the economic life at Woodbury may show a great economic change, as Mrs. Chitty has suggested, between the end of the Bronze Age and the beginning of the Early Iron Age. It may be that changes in natural environment have exerted some influence, but we must always remember the human domination of environment, and I think that we shall find that the change is much more technical and social than due to external environment.

*Dr. F. E. Zeuner.* With the beginning of the Iron Age the soil became decidedly damper. It was first recognized by Gams and Nordhagen that this change was sudden, and it has been connected with a rise in the water-table. A change of this kind is bound to have deeply affected agricultural conditions.

*Dr. Margaret Murray.* I should like to call attention to one problem which Mr. Hawkes has brought forward and that is that in the intercourse between East and West, we hear much of the influence of the East on the West and of the movements originating in the East. What we must not forget is the amount of influence the West has had on the East. That has been neglected in this Conference. Moreover, before we make any hard and fast systems in Chronology, we must remember that there is the whole of that great civilization of India and that vast country of China, the archaeology of which has hardly been touched as yet.

*Mr. Safrastian.* As an Armenian and Oriental impregnated with the old-world faith and outlook, the phenomena produced by the *homo oeconomicus* in the West strike me as threatening to social stability. People in my country say the same prayers and speak the same words as our forefathers in the fourth or fifth century. Moral ideas and words have not changed in sense and implication. A factor making for this continuity is the influence of a hereditary priesthood conscious of its real duty.

*Professor Sulimirski.* The mechanism by which a culture changes is an important question to elucidate which we must make use of social, ethnological and anthropological materials. A very interesting example of it is within my own experience in south Poland. In the central part of it, on the foothills of the Carpathian mountains, there had been interesting folk-dress and agricultural implements which I remember from my childhood. Just before the present war we were endeavouring to obtain specimens of those dresses and implements. We failed, and thus I witnessed a complete change of the material culture of this part of my country within the period of forty years, caused mainly by the great change in economic and social conditions.

*Mr. Miles Burkitt.* One must not forget that striking changes in clothes and in other articles in common use may take place without there happening any real change in culture. At Norwich there is a house in which each room is furnished with different period furniture. The Middle Victorian room has an ornate clock on a plush-covered mantlepiece, a large glass bell covering skeleton leaves, and chairs of unspeakable shape. It is all there even down to the smell ! One would have said it belonged to an entirely different culture to our own, and yet there has been no drastic cultural break between the two periods.

*Mr. Hawkes' reply.* I am sure it will gratify the catholic tastes of my Museum chief, Mr. Kendrick, that a discussion begun by me on the Hallstatt period should be ended by Mr. Burkitt on the Victorian Revival.

It has certainly gratified me that intervening speakers should have responded so fully to my propositions. I am especially glad that Dr. Clark should have emphasized the dangers of trying to interpret a period like this on one line of evidence alone. Different sets of phenomena, archaeological, religious, linguistic, ethnological, are naturally hard to interpret in isolation ; but there will be new hope, if once they can be brought together.

# THE ROMAN EMPIRE AND THE BARBARIANS

## I. THE ROMAN EMPIRE AND THE ADJACENT PEOPLES.

*Chairman :* Dr. H. I. Bell, C.B., O.B.E., F.B.A., F.S.A.,
*Keeper of Manuscripts, British Museum*

*Speaker :* The Rev. M. P. Charlesworth, M.A., F.B.A., F.S.A.
*Laurence Reader in Ancient History in the University of Cambridge*

In this paper I am making some suggestions as to where I feel that archaeologists can help a historian of the Roman Empire in dealing with historical problems for the solution of which archaeological work is required. I cannot claim to speak for anyone but myself, but I do believe that most of the questions put to you could be answered by archaeological methods.

We have reached a period in this Conference when the Romans impinged directly (by conquest) or indirectly (by diplomacy or trade) upon the lives of men and women of the various Iron Age cultures : a question of cardinal importance is, what difference did this impact make to them ? I propose to split my queries into three groups, which may be roughly termed Civilian, Military and Geographical, and I do not include Art and Religion in my list, since I observe present some high authorities upon them who can speak for and about them.

### 1. Civilian

(*a*) Considering the great importance of the things that people eat and drink, I would ask did Rome bring any new agricultural methods to these peoples ? Mr. Curwen has shown that the Roman conquest of Britain enlarged the amount of land ploughed up by the villagers of the Sussex downs, and gave them bigger and better fields. What effect did Rome have upon other Celtic field-systems, in Gaul, in Pannonia or Dacia ?

(*b*) Did Rome introduce new varieties of fruit and vegetable and cereals ? We have literary authority (Pliny, *N.H.*XV, 102), for the introduction of the cherry here in the first century ; observing that there is the same Latin base underlying the names of many fruits and vegetables in French, Flemish, German, Spanish and Welsh, we must ask whether these were new imports and whether archaeologists find any traces of these in excavation.

(*c*) Did Rome introduce any improvement or alteration in the breeds of horses or sheep or cattle ? The work of Mr. Curle at Newstead suggested that new breeds of horses were introduced by the cavalrymen stationed there. We know that some Romans at least were interested in improving the breeds of their sheep (Columella, VII, 2) ; what indications can you give us from your excavations ?

(*d*) Did Rome introduce any improvements in tools and implements? Six Roman ploughshares were found at Altenberg, near Cassel, possibly (as Mrs. Brogan suggested) a travelling merchant's stock. Do archaeologists observe any imitation of Roman tools in (say) Norway or north Germany?

Again, noting the adoption of building-words in Flemish and German and Welsh, we must ask whether you observe any trace of more advanced building methods in non-Roman territory, of attempts at bridges or aqueducts. In Ireland Cormac mac Airt had to send to a Roman province when he wanted a craftsman to build him a water-mill; did no other craftsmen stray into non-Roman lands to work for chieftains? Are there any traces of attempts at road-making in the Roman fashion, at something better than mere trackways? Are all made roads either "Roman" or "medieval"?

In commerce scholars have noted a great trade from the Empire to the North and North-East in cauldrons, buckets, pails and glass. Are any native imitations observable? (Mrs. Brogan has called our attention to Vandal pottery apparently imitating Roman glass.) Ekholm has shown that there is a division in the distribution of vessels in Norway, one kind along the Western coast, the other to the East. Why this curious division? If it is due to fashion, what caused the fashion? Shetelig and Falk (p. 313) have shown that Iron Age weights in Norway were based on the Roman *denarius*; do you find traces of similar influence in other countries?

To turn to the category of things military: here we can conveniently divide into Roman and non-Roman.

(*a*) Roman. Among districts needing more thorough exploration I would mention first that between the rivers Theiss and Danube, flying over which Mr. Crawford noted three hitherto unknown Roman camps; secondly, Slovakia, in the area of the rivers Hron, Vah and Ipel (Gran, Waag and Eipel). Can we be sure there are no Roman camps to find there? The famous victory monument at Trenčin, on the Vah, and the discovery of a camp at Mušov, near Brno, about fifteen years ago, suggest that there still may be surprises awaiting the archaeologist and historian. Thirdly, I would mention the region of the eastern Carpathians, with its passes and fortresses and earthworks beyond them. Here, if any success is to be attained, international co-operation will most certainly be needed, in an area where Hungary, Poland, Russia and Roumania are all concerned.

(*b*) Non-Roman. Here it is reasonable to ask what effect did Romans or Roman-trained natives have upon barbarian military science. Think of the line of small Roman garrisons stretching from Kerch, by Sevastopol and Akkerman and Barboçi, to Poiana and over the Oituz Pass; did not this have some influence upon native customs? At Bosporus the native levies were armed in the Roman fashion; at Poiana (if I read the evidence aright) Roman soldiers were apparently quartered in the middle of a native settlement. Do archaeologists discern any influence of Roman weapons upon native, of Roman military architecture upon native camps or fortifications systems? It has been said that the Black Pig's Dyke, in

north-eastern Ireland, shows such knowledge of Roman fortification as might have been gained from one who had seen the Roman Wall *from the outside* ; did returned native soldiers, who had served in Roman forces, learn no more than that ? And what of the curious site at Trelleborg in Denmark, where the regular plan with its four gates suggested a Roman prototype ? (*Antiq.*, XIV, 1940.)

I come now to my third group, Geographical. Let me say at once that one of the greatest needs of the present is a really good up-to-date edition of Ptolemy's *Geography*, complete. Collaboration between scholars, archaeologists and philologists would be essential, and it would take time, but the work must be done, for an eighty-year-old edition of Ptolemy is sadly antiquated in these days.

Apart, too, from books and texts, we ought to look more closely not only at camps and earthworks, but also at civil settlements, such places as the Romans called *commercia* (Pliny, *N.H.*XXXVII, 45) of which we have an instance at the mouth of the river Hron. May not future excavation discover something at (say) Kalisz, or near Elbing ? We need the study of coin-hoards and finds, such as Ondrouch's study of the Vyskovče find, and more news of Castrul de la Poiana, which Pârvan first brought to notice.

Here, too, though with many apologies, I must bring the Aestii on the scene. Though Tacitus declares that their language was " nearer to the British " (*Germ.* 45), nearly all modern scholars pooh-pooh the remark. But Tacitus had informants who could apparently distinguish between the British and Gaulish varieties of Celtic (cf. *Germ.*, 43), and these informants may well have been Romano-Gallic or Romano-British merchants. What I wish, in this connection, to emphasize is the large number of apparently Celtic river and place names which appear in Ptolemy in the region east and west of the Vistula. Did the Celts penetrate to this region ? Can you find any traces of them in your excavations ? I think that we should examine, too, the possibility that it was the Celts who acted as intermediaries between the Romans and the northern barbarians, by trade and diplomacy, filling in the west and north to some extent the same role as the Greeks played in the east and the south. It is a possibility on which I should like to hear opinions. For example, from what sort of people did Ptolemy derive his information about the Baltic regions ?

But I must not overrun the limits of brevity imposed on me. In conclusion I want to stress certain aspects of our work, above all the importance of co-operation : History, Archaeology and Philology are not rivals, but complementary to each other ; a refusal to work together will be fatal.

First, the importance of not believing too readily what we are assured, but going and looking. When I was younger it was a " known fact " that southern and south-western Scotland had not been occupied by the Romans ; thanks to the labours of Mr. Crawford and Dr. St. Joseph and Mr. Richmond and the Glasgow Archaeological Society the story is now a very different one. But the same may be applicable to northern Scotland, and possibly to Silesia, southern Poland and Bessarabia ; at any rate it is well worth bearing in mind.

Secondly, the importance of a thorough search of earlier records. Old travel diaries, records, statistical surveys often contain hints and suggestions which may be of great value to an intending excavator.

Thirdly, the importance of calling in Place-Name study to help. Ptolemaic names—such as Victoria, Felicia, Orrea (Horrea), names in -chester or -caster, in Caer- or Car-, places with such names as Römerstadt (Rymarov), all need thorough investigation. Quite apart from the volumes of the English Place-Name Society I can think of works by Professor Ekwall, Professor W. J. Watson, and Professor Albert Dauzat, which contain matter of great importance for any investigator, which he will neglect at his peril.

Finally, let me emphasize the importance, and indeed the urgency of a closer connection between our branches of knowledge, and of a friendly sharing in the results of our work.

## II. THE SLAVONIC EXPANSION

*Speaker :* Miss Edith Stiassna, Ph.D.,
*of the University of Prague*

The problem of the origin of the Slavonic peoples has led to many controversies. It is indeed remarkable that archaeology has for so long failed to identify beyond dispute the prehistoric stages of their development and to connect these reliably with their early historic period in the beginning of the Middle Ages, when documentary evidence, vague and inaccurate as it often is, first becomes available.

One leading theory has been, that the forefathers of the Slavs may be recognized in the people of the Lausitz or Lusatian culture of the Middle and Late Bronze Ages, located originally north and north-east of the mountain frontiers of Bohemia, between the Oder and the Polish Bug, and spreading most notably into Bohemia itself and into Moravia. Even German archaeologists, in earlier times, were of the opinion that this Lusatian Urnfield culture was Slavonic. But R. Virchow in the seventies of the last century denied this, and from then onwards others followed him, so that the opinion standardized by G. Kossinna now prevails in Germany, that the " Lausitz people " were Illyrians.

Among the older Czech archaeologists, J. L. Píč was one of the first who tried to prove the Lusatian Urnfield people to have been Slav, by tracing a connected development from their culture to that of the so-called Ring-Forts, characteristic of the same region in the post-Roman Iron Age, which is Slav beyond reasonable doubt. K. Buchtela had no hesitation in identifying the same Urnfield people in Bohemia, from their typical cremation-graves, with the Slavs of the tenth-twelfth centuries A.D. there, whose flat graves succeeded to a tradition of similar cremation-graves, belonging to this post-Roman Iron Age culture. L. Niederle, however,

who at first agreed with Píč, came later on to doubt the correctness of this theory of continuity ; and while not excluding as impossible, or even as improbable, the belief that the Bronze Age urnfields in Bohemia and Moravia were of Slav origin, he strongly emphasized the need for caution in the matter.

A. Stocký and J. Schránil made this caution more precise. For the theory of continuity, they could point to the identity of burial rite, namely cremation, between the Urnfield people and the post-Roman Slavs, and likewise to the demonstrable fact of an unbroken development in Bohemia and Moravia from the Lusatian to the Bylany-Plátenice culture of the Early Iron Age or Hallstatt period. But they both pointed out that from that point onward, for Bohemia at any rate, the continuity could not be safely proved. In a study published in 1933, Schránil went further, and pronounced the Slav origin of these urnfields to be an absolutely untenable assumption, since the Urnfield culture in Bohemia and Moravia developed into the Bylany-Plátenice culture only to end about 400 B.C., with the earliest La Tène period, leaving a gap of some four centuries before the first reappearance of cremation-graves, in the period contemporary with the Roman Empire. And it is moreover certain, as has been illustrated recently by new discoveries on the site of the latter period near Mušov in Moravia, that the cremating tribes who about the beginning of the Christian era colonized Bohemia and Moravia were Germanic, and that they remained there until the fourth century A.D. Schránil thus left the whole question of Slav origins as an unsolved problem. Moreover, he did not agree either with the alternative view, latterly more prevalent owing to its adoption in Germany as remarked above, that the Lusatian Urnfield people were Illyrians, or otherwise Thracians.

In Poland, however, the position is somewhat different. Most Polish archaeologists believe the Lusatian people to be the ancestral Slavs. In his reports on the excavation of the Early Iron Age settlement at Biskupin in western Poland (summary : *Antiquity*, xii, September, 1938, 311-317), J. Kostrewski has drawn attention to the apparent continuity of settlement in this area, which was inhabited in the Bronze and Early Iron Ages by the Lusatian Urnfield people, and in early historic times (as since) by Slavs. He observes in particular, that in both periods the population made a great use of horn and bone implements, and also established strongholds on low-lying ground exposed to inundation, of which Biskupin itself is an example. These facts are certainly striking but more discoveries still seem to be needed, if the theory is to be consolidated by final proof.

So far, we have considered the approaches to the problem that have been made from the side of prehistoric archaeology. But the question of the original homeland of the Slavs has also been approached from the side of linguistic studies and of the evidence of ancient historians and geographers. The ultimate goal is, of course, the same ; the difficulties on both sides are inter-connected, and solutions must of necessity be inter-dependent. Since the first indisputable mention of Slavs in recorded history dates only from the sixth century A.D., it has been necessary on this side to apply other methods, above all linguistic and geographical,

to supplement the scanty historical data and such archaeological evidence as has been available.

The first task of linguistics, with geography, is to place the Slavs within the original territory of the peoples of Indo-European language ; for to this great language-group, of course, the Slavs belong. All the evidence which geography, as also history and archaeology, can supply, points to a region somewhere between the Vistula and Dnieper. From a linguistic consideration of the flora of central and eastern Europe, one very interesting fact emerges which had been invoked to aid in establishing at least one part of the Slavonic homeland. The common Indo-European root-word for the beech-tree cannot be found to have existed in any of the Slav languages. The Slav word *buk* is obviously a later formation derived from the German *Buche*. This has been taken to indicate that the beech did not grow in the Slavonic homeland, which will therefore have lain in eastern Europe, east of a line from Königsberg to the estuary of the Danube, which does not know this tree. If the ancient distribution of the beech was similar to the modern, this argument can reinforce the many other considerations which have led to a theoretical delimiting of the Slavonic homeland within the following boundaries : to the west, the upper and middle Vistula ; to the east, the middle Dnieper with the Pripet and the lower Desna ; to the south the upper reaches of the Sereth, Pruth and Dniester, and thence eastward to the Bug. I must, however, point out that the western border may lie as far west as the Oder or even the Elbe, as was indicated by Professor Sulimirski in his recent lecture to the Royal Anthropological Institute. This contention will be reinforced by the archaeological arguments mentioned above in connection with the prehistoric site at Biskupin, if the validity of these is confirmed by the further archaeological research, which we have seen to be required to establish with certainty the Slavonic origin of the Lusatian Urnfield culture. In other words, the defined territory represents anyhow eastern Poland, southern White Russia, Podolia and Volhynia, and may extend also into western Poland and present-day eastern Germany. An older theory which assumed the Danube valley to be the first settlement-area of the Slavs has now been abandoned. Equally, it has been recognized as impossible to assume that the original Slavonic territory has a southern boundary in the Carpathians and the Black Sea. For at the beginning of our era there is no sign of any Slavs on the Black Sea, while, as is well known, Herodotus describes the Scyths as in his day inhabiting those regions, and earlier again, the Cimmerians.

The Venedi, Venadi or Veneti, however, described by Pliny, Tacitus, and Ptolemy as inhabiting an area extending beyond the Vistula, between the Fenni east of the Baltic, and the Peucini (or Bastarnae) east of the Carpathians, seem definitely to be the ancestors of the Slavs. This will mean that they were still in their presumed original home during the first centuries A.D. But it is not until the sixth century A.D. that the historian Jordanes identifies them as the Winidae —in German, Wenden—the " Wends ", or in other words the Slavs. In the German view referred to above, this name Veneti is explained as transferred to

them by their Germanic neighbours from the Illyrians who originally bore it, that is, the Lusatian Urnfield people. But we have already seen the alternative view to be, that the latter people were themselves Slavonic.

It is in any case generally agreed, that the later expansion of the Slavs, who then split up into different groups, took its start from this region, where they were said to have lived united for a considerable time beforehand.

This Slavonic Migration-Period, in its turn, offers considerable difficulties, inasmuch as it cannot be established with certainty when exactly it began. The Slavs invaded vast countries in the south, west and east, and had a long way to go to reach them. We might safely assume, therefore, that they did not all remain in their home country until the sixth century, but that some at least of them left it at an earlier date. Here we can only outline the picture of the expanding Slavs briefly, as follows, dividing them into the southern, western, and eastern Slavs.

The south Slavs, in historical times, are found well established in the south of the Carpathians, in Hungary, in the eastern Alps, and in the Balkan Peninsula. Their arrival on the Middle Danube must have fallen after the first but not after the fourth century. Their occupation of the Balkans took place not before the sixth century, and at the end of the seventh it was complete, including the overrunning of Macedonia and Greece. The tribes belonging to this branch of the Slavs are the later Slovenes, Croats, and Serbs, along with the (originally non-Slav) Bulgarians. The western Slavs, after leaving their native land, went westward towards the Oder and Elbe, to the Saale, and to the south-west beyond the Middle Danube to Bavaria of to-day. This movement seems to have finished before the end of the first millennium A.D. They were the Czechs and the Slovaks, the " Lusatian Serbs " or Sorbs, the Elbe-Slavs, Pomeranians and Poles. And finally, the territory of the eastern Slavs was the eastern part of their original settlement area, i.e. the region of the rivers Pripet, Beresina and Desna, and of Volhynia. In the sixth and seventh centuries they advanced northward to Lake Ilmen, eastward to the Don and the Sea of Azov, and southward to the Black Sea between the Dnieper and the Danube. And they were the Russians.

I have now arrived at a period, reckoned by Slav scholars as still prehistoric, but beginning only in the sixth century, when archaeological material becomes available, and reveals something of the cultural life of the Slavs. This is the phase called the Ring-Fort period, taking its name from the characteristic form of settlement. It is divided into the Early Ring-Fort period, from the sixth to the ninth century, and the Later, also called the " Chieftain " period, from the tenth and over the eleventh century. The ring-forts were more or less circular fortified settlements, on sites protected by natural advantages, and surrounded by defensive ditches and ramparts. The sites are usually hill-tops, but may also be in marshy land near rivers, and so protected by water. They were used for three purposes ; the purely military purpose of fortresses, that of refuges for the whole people in time of danger from enemy invasion, and lastly that of official seats or " capitals " for the chieftains. Since as a rule the only archaeological finds in them are

fragments of pottery, animal bone, and iron, we can draw the conclusion that they were not occupied by inhabitants in permanent residence. The graves of the Earlier Ring-Fort period are cremation-graves, placed in low tumuli ; but in the later or " Chieftain " period the burial-rite changes : at its beginning inhumation appears side by side with cremation, and thereafter inhumation-graves only are found, indicating the introduction of Christianity.

Here ends my journey, which has been long and may have seemed weary and laborious. I have tried to be constructive, but you will have seen that at present the most vital questions of the origin of the Slavs involve their students in assumptions which still remain to be proved right or wrong by fresh archaeological discoveries and more co-ordinated research.

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L. Niederle, (Note : L. Niederle is the *standard* work on the Slavs of the post-Roman Iron Age.) *Slovanské starožitnosti*, vol. I, II, III (Prague, 1902-1919) ; *Manuel de l'Antiquité Slave*, vol. I (Paris, 1923) ; *Poznámky kotázce slovanskosti našich polí popelnicových* (Památky Archaeologické, vol. 26, 1914, pp. 181 ff.).

For Polish literature, it is sufficient here to refer to Prof. Sulimirski's paper shortly to be published in the *Journal of the Royal Anthropological Institute*.

## Discussion

*The Chairman (Dr. Bell).* The two papers have stressed the need for the co-operation of the historian and the archaeologist. Each can make to the other a contribution to the enrichment of the other.

*Professor Sulimirski.* Taking the second paper first, one must stress the great difficulties involved in the question of the origin of the Slavs. The archaeologist needs, in trying to solve it, the co-operation not only of the historian, but also of those who are concerned with linguistics and physical anthropology. In ancient graves of the oldest Slavs in Jugoslavia and in Poland are found skulls of Nordic type. It might be a proof that they were derived from the north. I do not wish to make any assertion on this point, but merely to call attention to it. It seems to me that about the time of the Roman Empire, or some centuries later, there appear to have been Germanic elements in the south of Poland, and a possible explanation is that the two peoples lived together, in the same country, forming two separate classes, in the same manner as the Franks in France, who were later absorbed.

I was much impressed by Professor Charlesworth's questions, though I am not a specialist in the Roman period. The Romans, however, had penetrated as far as Poland, and there were thousands of Roman coins and some fibulae in Poland.

In my own area in central Europe the number of archaeologists is very small, and there are a multitude of questions they will be called on to answer. They will also be handicapped by financial difficulties.

*Mrs. Dobson.* There is a great difficulty in disentangling the influence of Rome itself from that of Christianity, which went on uninterrupted. A point of interest is the way in which the natives adopted Roman ways, with resultant fusion of cultures, as evidenced by finds in Wookey Hole and elsewhere, and such things as dragonesque brooches, which are Celtic in pattern. In Celtic mirrors there was a double influence visible. A problem which needs solution is that of the existence of Roman vineyards in England.

*Professor Minns.* With reference to the problems of the Slav migrations, the most important thing to remember is that two nations can occupy the same place. Points to which I would draw attention are the Antes, ancestors of the Russians, who overran Europe in the first century, and the Russian theory that the Slavs were derived from the Pripet area. There are Ring Forts in the Ukraine.

The evidence concerning the Beech trees should not be given too much importance. I do not believe in it. The proportion of botanists in a community is too small to give reliable evidence. Some people could even call pines hemlock. It is easy to confuse botanical names.

Early legends may give useful evidence, and it is possible that one can identify the Slavs in Herodotus. Perhaps the human half of the " Werewolves " was Slavonic. And again, Professor Childe's Danubians might be Slav ancestors.

# SUMMING UP

*Chairman :* Professor S. R. K. Glanville, M.A., F.S.A.,
*Edwards Professor of Egyptology, University of London*

*Speaker :* C. F. C. Hawkes, Esq., M.A., F.S.A.,
*Assistant Keeper of British and Medieval Antiquities, British Museum*

After referring with regret to the absence of Sir Cyril Fox, in whose stead it had fallen on him to present this summing-up of the proceedings of the Conference, Mr. Hawkes said :

I cannot expect to be able to recall more than a selection, which may well seem to you a rambling one, from the multitude of important sayings and telling points which this Conference has offered to our memories. I can only hope that a few thoughts suggested to me by some of them may stimulate you to retain and treasure many others in your minds besides.

But I think first of all we are truly entitled to say that the Conference has been a great success. It has surely justified the optimism of the little band of representatives of half a dozen nations, whom Miss Kenyon six months ago gathered around her to devise ways and means for holding it. True, the dynamic events of this summer, the procession of which is still unrolling, have caused it to be summoned only after a postponement, and then at short notice ; true, even that was something of an act of faith. But your response has shown that you were determined it should succeed ; and by your attendance both yesterday and to-day in such splendid numbers, and by the enthusiasm which you have displayed throughout the sessions, you have ensured its success yourselves.

When Sir Cyril Fox summed up the Conference on the Future of Archaeology that was held at this Institute last year, the subject he turned to first was the nature of archaeology in general—the conceptual frame, as he put it, which unifies our activities. And yesterday Professor Childe, picking up the same subject in his very inspiring Introductory Address, seemed to me to epitomize the matter most happily by declaring that in its essential nature Archaeology is a true University. Its material can be brought to engage everyone's attention, and everyone can be interested in its results. For the material consists of facts, which lie open (unless barred wilfully or by neglect ; but with no insurmountable restriction of language) both to the plain man everywhere, and to a diversity of types of student of whom the archaeologist *sensu stricto* is only one. And the results will be the more widely interesting, the more widely advantage of that is taken. The nature of archaeology indeed makes it a centre of convergence for both a human and a scientific range of interests, which it brings together to explore all the past but recoverable works of man as a social being. The field is immense, and ultimately world-wide. We

indeed have been considering it for Europe and the neighbouring regions only, but we have found plenty to think about ; and plenty, moreover, to prompt us to the idea of international co-operation for giving our thoughts effect.

To thought and to aspiration alike, Professor Childe gave a powerful lead ; and for period after period the speakers following him have brought outstanding problems before us, and have again and again proposed international team-work of some kind or other as necessary to their solution. I shall try to remind you of a few of them.

Of the Palaeolithic first of all, and of its chronology in particular, Dr. Zeuner had much to say ; and I join most readily in endorsing his final commendation of five especial problems for research in that great field. I was delighted, too, to hear Dr. Oakley say that in the last resort there it is for geology, with its attendant sciences, to date the archaeologist's material, and not for the archaeologist, in any right of his own, to sell things to geology as self-certified dating-fossils. I am sure that is a very important truth.

Now Dr. Clark did well to point out, when his turn came, that chronology is only a frame, and that inside the frame is the subsistence and society of human life. But our knowledge of that life is obtained by analysing the material remains left by the communities in which it was lived, and working our results inductively into a scheme of cultures, each one the expression of a distinctive pattern of living. And the relation of all that to measurable time, as also to measurable space, is essential to our conception of how it makes sense. In the Palaeolithic, the matter was for long disposed of very simply. Each of the epochs making up the period was thought to mark a stage in a uniform and universal cultural evolution. It is only within the last twenty years, or barely the last dozen years even, that we have become accustomed to the idea that Palaeolithic cultures had a variable range in time and space, like those of later periods. The coming of this idea has been an event of radical importance in the progress of archaeology—and how radical we could easily apprehend by comparing Dr. Zeuner's propositions with the old theory of universal succession : Chellean, Acheulean, Mousterian, and so on.

But in welcoming the new conception as we all must do, we should I think be careful to keep clear about its implications. In geology, the spread of each period's new type-fossils is considered as instantaneous. When one is dealing with geological time, that is all very well. Equally so, in archaeology from the Neolithic onwards, is the notion of a patch-work of cultures neighbouring one another in time and space ; for then one is reckoning time by centuries or less, just as in the period-maps of an historical atlas. But to project this notion into a Palaeolithic where time is to be reckoned by tens of thousands of years is a proceeding which ought to be thought about very carefully. Take the encroachment of Aurignacians, for instance, upon the Final Levallois habitat in western Europe. If we are to accept it as *not* geologically instantaneous, but as moving gradually within our little Continent through several geological horizons, then it was much too slow to be at all easily comparable with a Neolithic or Bronze Age migration ; or for a fairer comparison,

say, with the migrations of the Thule-culture Eskimo. To chart the movements of tiny groups of (one supposes) highly mobile Upper Palaeolithic hunters, over distances of a mere few hundred miles, the modern scientific chronology is a very long scale indeed ; and I am sure Dr. Zeuner will agree that students of humanity ought to make quite sure, when they adopt it, that they realize what they are in for. The same applies, let me add, to students of Upper Palaeolithic art.

For the Mesolithic—but of course his words have a wide application to other periods too—Dr. Clark spoke compellingly of the importance of co-operation between archaeology and the strictly natural sciences. Indeed, he gave this matter what is to many no doubt a new turn, by demonstrating that it is not simply a matter of getting man properly placed in his environment, but also of recognizing man as himself an actor upon that environment ; or rather, of recognizing man's work as one of the agencies in the operation of nature. Perhaps some of you have occasionally felt that the modern cordiality of its entente with natural science is making archaeology inhuman. I would say just the contrary : it is archaeology conducted in a private vacuum that is inhuman—inhuman because unreal. At the same time it will not help the entente if archaeologists undervalue their own proper side of the business ; and we must all have been touched, I think, when after Dr. Clark's paper and my wife's, Mr. Burkitt rose and pleaded with his two ex-pupils in their brilliance not to forget the old grammar of their subject. Yet I would ask Mr. Burkitt to keep in good heart. In a subject that is really progressing, the most brilliant pupils are they who having learnt their grammar can make bold to revise it ; and thereafter, if time duly vindicates their boldness, the indiscretions of one generation will become the axioms of the next. Indeed, Mr. Burkitt himself made plain that his tutorial emotions yesterday were not unmixed with pride. And both in the Mesolithic and the Neolithic field, there is work ahead of which the accomplishment will I hope mix them with more.

The Mesolithic—to take that first—holds many problems still unsolved which it was not possible for speakers here to touch upon. In the last fifteen years, the classification of Mesolithic cultures has been so well worked out for northern Europe, and for much of central and parts of eastern Europe too, that I think it is time we turned our eyes more towards the south and west. After all, in the earlier days of Mesolithic research it was from the south-west that the main stream of microlith-using people was always said to have entered Europe, out of north Africa. Our Tardenoisian cultures have been traditionally derived from the north African Capsian. But what is the Capsian ? How much, or how little, does that too glibly quotable word denote ? In north Africa itself, granted, the French have latterly done a good deal to anatomize it. But the process has yet to be applied to Spain ; and the whole question of the relation of immigrant to aboriginal elements in the Mesolithic of south-western and western Europe still stands before us. Moreover, we have to remember the outline of the same question in the south-east and east : there again we have something called Tardenoisian, but it seems to come in from southern Russia. Work on these things has been well

begun, both in Poland and the Soviet Union, but much more remains. I believe, too, that both in East and West, the subject is important not only for itself, but for the ethnological background it can give to later prehistory. In the east, above all, it is necessarily bound up with the whole problem of the origin of the peoples of Aryan or Indo-European speech. And both there and in the Mediterranean West, I fancy, the co-operation with natural science of which Dr. Clark spoke will be needed no less, than in the more northerly regions where it has done so much for Mesolithic studies already.

To turn next to the Neolithic, I do not think you will want me to take you again over the ground which my wife covered concerning the colonization of the Mediterranean and Atlantic West, where, among the problems with which this and the ensuing Early Metal Age confront us, those of the Megalith-building peoples stand out in such prominence, and yesterday attracted such attention. On the right and wrong ways to set about the typological study of Megalithic tombs, her thesis seemed to gain your approval ; and it moved Professor Childe to a sudden outburst of approbation, which surely deserves to rank as one of the Conference's big moments. But behind and beyond that, once the subject of the spread of Neolithic cultures is introduced, we have, of course, to remind ourselves that it consists simply of the earlier instalments of the whole long story of the spread of civilization from the Orient to the Occident, and of the Occident's response and reaction to it, of which so much of European prehistory is made up. The Neolithic world of Europe, not only in the regions of Megaliths but everywhere else as well, would not have responded and reacted as it did to the diffusion of civilization in the Bronze Age, had it not itself been the product of similar processes, though less complex ones, in its own day.

The south-western and western series of those was in some measure yesterday discussed. Of the south-eastern and Danubian series, with its manifold repercussions all round about in Europe, very little was spoken ; but I take that to be a silent testimonial to the success which has attended Professor Childe's work, these twenty years past, in making this great division of European prehistory known and appreciated amongst us. Moreover, the sequel and the result could be well seen when we considered the Bronze Age and the later prehistoric centuries to-day, for in discussing them we all felt confident of the nature of their Neolithic background, and could readily respond, in particular, to the propositions regarding continuity between Neolithic and later cultures, made during the day both for the eastern and the western parts of Europe which the Danube lands connect.

This idea of a substratum in the populations of ancient Europe, which, whatever its ultimate origins, has descended at least from the Neolithic, is of course not new. It is familiar in the west of our own island ; and it is familiar above all in the classical lands of the Mediterranean, where the peoples of the " Mediterranean Race " sustained the coming, in and before the dawn of history, of the warrior invaders from the north who made the Heroic Age. From their wide range of immediate homelands in east-central and eastern Europe or beyond, such warrior

invaders spread far and wide ; and the Neolithic patchwork was already crumpled, overfolded, and stratified within itself when the Oriental elements of Bronze Age civilization began to supervene.

On that momentous process Dr. Schaeffer gave us a fascinating discourse, and it provoked a discussion which I hope we shall see recorded in detail. I will not attempt to recapitulate either ; but I think you will all have been impressed with the value of his contributions to that Oriental chronology, which is of such importance for dating the major stages of our Bronze Age development. To European prehistory to-day the Near East has drawn nearer than ever, and for no period is this truer than for the Bronze Age. Moreover, its significance is not in the least diminished—indeed, it is enhanced—by recognizing the essential originality of the European response to Oriental inspiration. And in achieving that response, Europe created for itself the new patterns of culture, bringing things both new and old together, which characterize the Bronze Age in almost all of its various regions.

In the British Isles, the patterns became presently unified in the Middle Bronze Age culture of the " urn-folk ". In eastern Europe, Professor Sulimirski brought fresh light to us on the formation of the Lausitz or Lusatian culture, and of Volhynian and Thraco-Cimmerian cultures beside and beyond it. And just as in the culture of our British " urn-folk " we recognize a sturdy substratum descending from the Peterborough Neolithic, so in those eastern regions he was able to detect substrata descending from their old pottery-painting Neolithic peasantries. In central and west-central Europe, a similar though more complex ethnic and cultural stratification worked out into the two major patterns of the Tumulus and the Urnfield cultures. In all these developments we have encountered problems calling for our international co-operation. And this is true above all of the great Urnfield expansion of the later Bronze Age, which affected both east and west and northern Europe too, Italy likewise and even Spain, and the Balkans also and the region of the north Aegean, where its story crosses with that of the break-up of the Mycenean order.

So, then, we reach the great period of transition from the Bronze to the Iron Age, and the Iron Age itself, with their further array of problems. But again, these have appeared to us in much the same broad terms, of new initiatives and movements against a background of old and deep stratifications of humanity and culture, and of civilizing influence from the east balanced by fresh and spontaneous reaction in Europe. In fact, I think one of the most striking things about the Conference has been the impression it has given one of the fundamental and recurring character of these major terms in the expression of the European story. The problems with which the later chapters of the story confront us to-day owe much of their difficulty not to the inherent character of our materials for their solution, but to the patchiness and inequality of our work on them in the different countries concerned, and above all to the imperfect correlation of results between one country and another. I mentioned Italy as a case in point which is at present particularly glaring ; but there are others, and I think everyone will take away

from this Conference a clear conviction of the vital importance of unstinted international contacts in the new phase of our researches which will open after the war.

No doubt free play for such contacts is something that will have to be aimed at, and not just assumed. Europe is very sick, and the recovery of its health, intellectual and spiritual no less than physical, will only come gradually, by striving and by patience. The question of a remedy for Germany, alone, is obviously one of appalling difficulty; and east, west, north and south in Europe we have all to get to know each other better. In isolation, amid a state of affairs like this, such folk as archaeologists are plainly weak. The individual may well feel himself absurdly so. But it is from the individual's spirit that all concerted effort really springs ; and with us, the spirit at least should be willing. For while we can safely say that most of us respond readily to the attraction of archaeology in our home countries, we surely can say more : there can be few of us, I suggest, who would wantonly disown the attraction of archaeology abroad. Indeed, if your home country lies north of the Alps, the attraction of archaeology in a Mediterranean country has a force and a fascination all its own ; above all, perhaps, in the Greek and Aegean lands for which Miss Lorimer spoke so feelingly and so fitly in her few words this morning.

But archaeology out there has in the past had two apparent disadvantages : and these I will mention, because I want so to see them reduced to their narrowest possible limits in the future. One is of a material order, and belongs to the pursuit of archaeology—and of most other things—not only in Greece but anywhere else that is a long way from home : the travelling, whatever the subsistence, costs too much for some people, even if they can afford the time. But once post-war Europe has got into its stride, travelling may be easier than it used to be, and increasing demand should make it cheaper. Of the increasing demand at least I have no doubt ; and what the popularizing of travel should do for the betterment of European relations in general, it should at all events help to do for the betterment of European archaeology. It should make expeditions easier for all manner of purposes, both for discovery, survey, excavation, or simply visiting sites and monuments and museums, and for the occasions of the individual archaeologist or student on his own, desiring for example to check up on foreign material imperfectly known to him by going to see it for himself. But let us in Britain not be unready to welcome such visitors ourselves. Most of our material will be very imperfectly known to them. And if any air-travel is going to be within the reach of an archaeologist's pocket, so much the better for us all. Think of finding a Bronze Age trinket one week in a Wiltshire barrow, and flying the next week to match it in the Candia Museum : what could be more exciting to the mind, or more exhilarating to the soul ? Of the possibilities of archaeological air-survey I will not here speak ; but clearly there is much that could be said about that also.

The second disadvantage of archaeology in the classical lands of the south of which I was just now thinking, and in the ancient east, is of a philosophical order, and although when ideally considered it will vanish, or indeed turn into an

ideal advantage, it has a real existence none the less. The lands of barbaric antiquity, in the west and the north and the east of Europe, usually make easier archaeology, because theirs were for so long the simpler cultures. In the lands of civilized, or comparatively civilized antiquity, the cultures were more complex, and the archaeologist's problems are correspondingly more bristling and formidable, and more difficult to study. Epigraphic, literary, and linguistic evidence may bring in all kinds of complications, unknown to the world of Peterborough or Skara Brae simplicity, the dumb purity of Windmill Hill. Yet all this is a matter of degree. All archaeological problems call for team work in science and scholarship, and we saw more than once this afternoon how some at least of such complications beset the study of barbaric antiquity in Europe no less than of civilized. A man who goes to school in the simpler regions can pass on to mastery in the more complex : look at Dr. Schaeffer. But the truth is that all archaeology to-day is difficult. To say that Aegean or Oriental archaeology is conspicuously difficult is to vindicate both its fascination and its importance : it is through its difficulty that its ideal advantage can be realized. But it shares that difficulty, to a greater or less extent, with modern archaeology everywhere. The whole subject is difficult by nature. And that, surely, is precisely why we like it. I think we are extremely gallant to devote our lives to it at all, but the fact remains undeniable that we enjoy it immensely. You have shown that yourselves at this Conference, by an enthusiastic cheerfulness that has shone throughout all our sessions. So I do not think we need worry much about this matter of difficulty, since we evidently mean to show the post-war world that difficulties are the sort of thing that we exist for.

And why ? Because we want archaeology to be true to life, the investigation of which is its proper business ; and life is difficult, and in one way or another—civilized or barbaric or savage—it almost always has been. Moreover, the chain of life is continuous ; and to investigate it successfully we have to link its various manifestations together, the simpler with the more complicated, the wholly prehistoric with the partly historic, and that again with the fuller manifestations given us by history with its written evidence.

Now here, I think, we can congratulate ourselves upon the contribution made to this Conference by Mr. Charlesworth. He spoke to us from the standpoint of Ancient History, and he showed us that in their professional concern with classical antiquity, above all with the Roman Empire, ancient historians can be extremely interested in our affairs ; not only with the historic and semi-historic but with the prehistoric archaeology of Europe, and not only inside but outside the lines of the Imperial frontier. Archaeology can indeed help history by taking its students over from words to things ; and the questions about the history of things which he propounded to us are surely just the sort of questions which we as archaeologists should be propounding to ourselves. True, the documented historian has a wider experience to prompt him to such questions. But this seems to me to improve the fun : it shows it, in fact, to be the reflection of something seriously and excitingly

important. We have an essential part to play in the wider business of history. Even when out of contact with documentary history altogether, archaeology must join hands with a whole series of other pursuits, as we have already seen. Contact with documentary history simply lengthens that series, and brings us presently to join hands with the entire range of studies which have a bearing upon human affairs.

As students and interpreters of the things which man has made and done upon the earth, we can claim that our contribution to those studies is indispensable and vital ; and this is a claim that is up to us to vindicate. As human doings become progressively more complicated with time, the archaeologist should not start to fall out of the line of their investigators. If he feels he is only really at home with prehistoric affairs, he should recall that there is no true division between prehistory and history. And if that was clear from our consideration of the centuries connecting the Roman Empire with the prehistoric world before it, how much clearer was it when Miss Stiassna took us over to the archaeological study of the Slavs, and made us consider the whole span of the centuries connecting the same prehistoric world with the Middle Ages ! The problems of continuity within that span are not peculiar to Slav studies, nor limited to eastern and east-central Europe, and I think her paper was most valuable to us not only in itself, as a lucid summary of a subject which here in the west has been too little known, but also for the stimulating comparisons that it must provoke between the issues that lie before Slav, and before Teutonic and Celtic archaeology. I only wish that there had been more time for those matters to be discussed. But at least we can have now no excuse for blinking the conclusion, that history and prehistory in Europe belong together, and that archaeology has everywhere essential work to do in both.

Essential : because through studying things it studies the people that made them. It is the people, the human communities whose solidarity has gone into the nations of Europe, that are our real objective. What and who are they ? Who are, who were the Europeans ? There have been bogus answers in plenty. The most terrible of them all is still struggling to stand against us in its home in Germany. But the true answer lies ultimately in the nature of humanity entire, which we may view in the three aspects of man the individual, man in society and man in his place in the environment of nature. Towards the truth that will unify those three, our subject can, in its just alliances, help to take us far. And I think we can look confidently forward to a sound and sane influence for it upon the natural and social and political philosophy of the future, provided we remember one thing—that through all his classifications and definitions and distributions and typology and analysis of material, of things, the archaeologist must see people ; he must see man. For man, when all is said and done, is his proper study.

And now, lastly, I want to remind you of the problem presented by man the archaeologist himself : the question of *Homo archaeologicus*. What of him ? How, above everything, is he going to make the contribution which we claim he can make to the post-war world by the practice of his craft, unless he is there in the necessary

force of man-power? Without that, and without the training which his craft requires, without greater organization and opportunity for it, and without the acceptance of his kind as a real working asset to the modern world community, the contribution will not be made. At present—good as it is to see so many people in this room—true archaeologists, whether professional or otherwise, are painfully few. We need recruits. Not half-baked enthusiasts, but serious and sensible people for whom archaeology is a real business in life. To create them, there must be education—education that will produce many minds fitted to appreciate the business, and enough minds trained to practise it. In our own country here, this is an anxious problem enough. Where war will have made so much worse havoc, material havoc and human, it will inevitably be far more anxious. Everywhere, the answer lies most obviously with Universities. Behind them, it lies also with the schools; and I believe that theirs is the fundamental level. This means—remember the dire plight out of which Europe has to struggle—that results can only be gathered slowly. And they will not be fit for gathering, unless we look to it and take good care of them meanwhile. But if we do, I think we need not despair. For were we able to do things in a hurry, we should run the risk of soon finding all to do again. And soon might already be too late.

Naturally I shall be pleased if in twenty years' time a conference of this kind comes to fill not this room but the Albert Hall. Yet I believe that for us archaeologists to be few, as we are in the world to-day, is to be a happy few. Yes, I do mean a band of brothers. Our band is small, but are not our opportunities great? There surely is our happiness. And there—the best hope we have—is our share of honour.

In conclusion, I must express on behalf of those present our thanks to the organizers of the Conference. Miss Kenyon and the responsible Committee have provided us with a most enjoyable two days of relaxation from our wartime pursuits, and we congratulate them for persevering with an enterprise which appeared at times most unlikely to come to fruition. The co-operation of archaeologists of so many allied countries in the organizing Committee is, we hope, a foretaste of our co-operation in archaeological enterprises after the war. To the Institute staff and the band of *ad hoc* assistants we must express our deep thanks for the arrangements conducive to the comfort of our meeting, above all to those responsible for the excellent food which sustained our arduous labours. To the speakers we would say that their words will guide us in the work which lies ahead of us, and we deeply appreciate the time and thought they have devoted to the preparation of such brilliant papers in spite of many heavy pre-occupations. Finally, to the Chairmen of the different sessions and to the contributors to the important discussions, we would express our gratitude for their share in a most successful enterprise.

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